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CIVIL ENGINEERING.

On the Organization of the Corps Imperial Des Ponts-et-Chaussées, in France. By GEORGE R. BURNELL, ESQ., C.E., F.G.S.

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(Continued from page 12)

THE ultimate consequence of the strict system of promotion that prevails in France is, that all that relates to the general direction of the public works of that country is obliged to be transacted through the body of the Ponts-et-Chaussées; and the civil engineers are there reduced to the rank of conductors of the industrial establishments, that are still allowed to be free from state control, although the civil engineers have proved themselves to be capable of directing the railways and other public works, with at least equal ability with their more fortunate rivals. The railways seemed, for a time, to be destined to form an exception to the general body of the works, for the late Mr. Locke and the English contractors were entrusted with the execution of the Paris and Havre and the Cherbourg lines; but it was found that the relations between the Minister of Public Works and the company were so numerous, and they comprehended so many questions which involved the rights of the Ponts-et-Chaussées to interfere with the working of the railways, that even upon these lines the government engineers have been employed. Indeed it is difficult to imagine any other course being long adopted in France; the whole service of that

country is so desperately centralized that the intervention of the state is felt in all things. Thus, the body of the Ponts-et-Chaussées, being always upon the spot, is necessarily employed for making the surveys for the lines of railway, roads, and canals, that may be thought necessary; and the organization of that body provides it with ample means of carrying out the projects that it conceives. It may be that the nation gains in the unity of views that is thus produced, and that many cases of competing lines may thus be avoided; but this advantage seems to me dearly purchased, when it is only obtained by the annihilation of the private enterprise of the whole country, which is the inevitable result of the French system. It has been pretended that the estimates that are prepared by the French engineers are better and more trustworthy than those that are made by their English brethren, and that a contractor would be more inclined to risk his fortune on the French preliminary surveys and estimates; but if we set aside the greater facilities that the French engineers have for making their calculations for the works that they are employed upon, the superior correctness of their estimates in works that are at all out of the common run may be very much questioned; and certainly they design their works in a manner that English engineers would consider to be very extravagant owing to the traditions that prevail in the body of the Ponts-et-Chaussées, who pride themselves rather upon not considering the commercial view of the question in their constructions. It is, however, to be observed that much of the merit that is thus due to the engineers of the government is owing to the admirable system upon which their accounts are kept; and the merit of the "comptabilité" that forms, so much and so justly, the boast of the engineers is, after all, due to the conducteurs, whom the regulations of the body consign to perpetual inferiority. The engineers, in fact, content themselves with the duty of organizing the manner in which the estimates and the definite accounts are to be prepared; they leave all the details to be worked out by the conducteurs, who fill the position of the Soodras in the "hierarchy" in which their chiefs play the part of the Brahmins. There is much more truth than appears at first sight, in this comparison with the Indian social arrangement, for the classes that are thus formed in the French body politic are as strictly defined as those of India, and there is nearly as much difficulty in passing from one rank to another. The distinction between the ingénieurs des Ponts-et-Chaussées and the conducteurs is, however, a permanent and ineffaceable one, that will always be a standing reproach to the system that would prevent a man from rising by his own merits to the foremost ranks of his profession.

Yet, although I am myself fully aware of the inconveniences of the French system of organizing the public works of that country; though I know the great expense that the nation is put to on account of the want of practical knowledge, and from the deficient technical education of the engineers that the government is obliged to employ, from the fact that no man can there rise from the ranks, let his merits be ever so great, and from the mania that the system fosters for constructing monuments that should last for ever; yet I have seen such confusion

and such waste occasioned by the system that prevails in England, especially in the appointment of government officials in similar matters that I should be almost inclined to prefer the French system of the Ponts-et-Chaussées to the ignorance and incompetence that prevail here in government offices. The engineers of the Ponts-et-Chaussées are thoroughly acquainted with the subjects they have to pronounce upon, and they all feel as though the reputation of their body were entrusted to their safe-keeping; whilst in England the nomination of the engineers of the government is entirely regulated by favor and caprice. Fortunately, the influence of the authorities that are appointed by the state is very small in England, for we still retain so much of the independence of habits and modes of thought that are the essential characteristics of local self-government, that the engineers named by the government can do little; but if the circumstances should change, and if the influence of the central government should be increased, it must become a serious question with us, whether the organization of the Ponts-et-Chaussées, that does not admit the nomination or the advancement of the favorites of the minister of the day, may not be the best that can be devised in the interest of the rate-payer. In a highly-centralized government, like that of France, the organization of the Ponts-et-Chaussées no doubt produces excellent results. It would be out of place here, as long as we retain our self-dependence and that love of true equality, that enables any man to rise according to his own merits; but it is the best system that has yet been thought of for securing the talent of the men and the independence of their judgment from the theories or the passions of the moment. There are several details in the organization of the service which might easily be altered (as, for instance, the fact that the engineers are only responsible for the acts that they may do in the discharge of their duty to the *Conseil d'Etat*, and the fact that the conducteurs cannot rise to the superior ranks of the profession), but, with these exceptions, there seems to be no reason why something like the organization of the Ponts-et-Chaussées should not answer very well with the gradual tendency to the development of centralization that is so manifestly gaining ground amongst ourselves. It would secure for the public at least the services of educated engineers, and put a stop to the curious nominations that we have sometimes witnessed in our public offices; added to which advantage would be the incidental one of raising the tone of the profession in matters connected with their own emoluments, which recent events have shown to be very low in England. Perhaps there is something that is opposed to the English notions of government in the existence of a body that is composed of officials, who are not in any way liable to the influence of the ministers of the day, either for their nomination or for their advancement, and there may be danger in thus creating a body that should have the control of the funds of the nation to the exclusion of the rest of their profession, and to the perpetual interference with the ingenuity of the great body of the public; but this is counter-balanced by the advantage of the absence of the influence of the minister. There is, of course, the disadvantage

of having to do with a board of theoretical men, who are likely to insist upon the observance of the strict scientific conditions that they conceive to regulate their pursuit, as was the case with the Ingénieurs des Ponts-et-Chaussées in the matter of the rate of the inclines for railways, which they for a long time insisted should be made with an inclination of 1 in 200, when the English engineers had proved that they might be executed at 1 in 60 without inconvenience. This is a drawback that will always attend an organized body of such a nature; but there must be set against it the advantage that this body procures in controlling the action of the government in the details of the practical application of the science. The minister is, indeed, quite powerless in cases which strictly belong to the carrying out of works that may be ordered; the Ingénieurs des Ponts-et-Chaussées alone can regulate the manner in which they are to be executed. The fact that the Minister of Public Works is a political appointment, and therefore is subject to the caprice of the parliamentry government, throws a great power into the hands of the Ponts-et-Chaussées, who are always in possession of the influence which they may derive from their position as distributors of government patronage, and as executing the works that the state may, through their advice, undertake; and this they have shown that they know how to use for securing their independence. It is desirable here to mention that the course adopted in the execution of public works in France is for the Minister of Public Works to submit to the legislature a project of law for their establishment, if they are of a nature to require a concession from the state, or to give rise to a large advance of public funds, or if they should require the application of the law of expropriation; in other cases, they may be the subject of simple ordinances, as in the case of municipal or departmental works. The projects for all of these undertakings must be accompanied by drawings, specifications, and estimates, which must be approved by the conseil that is specifically concerned in their execution, and they give rise to the production of a mass of papers and reports that are produced for them, and which are themselves perfectly bewildering. The worst of this method of entrusting the public works to the care of the state is, that there is no security that they shall be finished if once begun, provided the state be in want of funds, or it should have been found advisable to appropriate them to other uses. This last contingency has, it may be observed, already happened in several remarkable cases. For instance, the breakwater of Cherbourg, which cost the total sum of £2,400,000, had been mounted by the simple interest of the money during the time that it was in hand, to the sum of £8,400,000. The cost of the canals had been just tripled by the interest that had accumulated upon the sums invested in them before they were inaugurated throughout their length. Besides this there is always the danger and difficulty of the works being executed in an official manner, that gives rise to the inconvenience of reports and reference to the various offices of the state, in case there should be any reason to change any of the details of the system that may have been agreed upon at the first conception of the scheme. All the questions must be decided in the

Conseil des Ponts-et-Chaussées of Paris, and, setting aside for the moment the absence of local control that must thence issue, there must be a tendency in that body to impose there own style of work and their own modes of thought. In the cases of the works that are conceded to large public companies it might be supposed that the adminstrations of the latter would have sufficient power and influence to reduce the interference of the body of the Ponts-et-Chaussées to their legitimate bounds, but in all the concessions hitherto given there is left so wide a margin for the action of this body that they are practically all-powerful. The works are to be executed to their satisfaction, and they have a control over the workmen, the materials employed, and the manner in which the work is executed, so that they can stop the companies at any stage of their progress. Hitherto the force of public opinion has kept the engineers within the bounds of what may be considered their duty. It remains to be seen what will be the effect of this system when it is applied to a country fully accustomed to centralization, and debarred from the control of a free press, such as France is at the present day. The tendency of what is now passing in that country is to introduce the control of the engineers independent of the minsters of the day; to establish the system of the monopoly of classes that is perpetuated by the necessity for the pupils passing through the Ecole Polytechnique; and by the exclusion of the conducteurs and the civil engineers, to concentrate the money and the patronage of the state entirely in the hands of the engineers of the Ponts-de-Chaussées. I think this must eventually prove to be a wrong policy, though it certainly tends to raise the character and position of the engineers; and that France will, sooner or later, find herself, in this respect, in the trammels of a system of castes that will effectually destroy all originality of thought and action.

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Discussion.—Mr. Haywood (Engineer to the City Sewer Commission) said he did not gather from the paper that Mr. Burnell held any conclusive opinion as to whether the system in France was better than that in England, or the reverse. With a great deal Mr. Burnell had said he fully concurred, but the root of the whole question of the organization in this country of a corps corresponding to that of the Ponts-et-Chaussées lay in the political condition and habits of the people. Whether it would be desirable or not, there was little probability of its being done in our time. He must confess that being tolerably well acquainted with France and her institutions, he never went there without regretting that some system could not be established in this country by which more unity of action in engineering matters could be obtained especially with regard to the metropolis; but he could not see any intermediate condition between the present local-government system and the institution of a corps somewhat similar to that of the Ponts-et-Chaussées. If that corps was tested by the grandeur of its works, it was far in advance of the English; on the other hand, if the results were tested by the activity of commercial enterprise; then the French system was far behind ours. They had then to choose between the two. With

regard to the works of the engineers in France, his conclusion was that their aqueducts, docks, high roads, and canals, were laid out better than our own, and also that the workmanship in them was superior to ours. With regard to the originality of their works he did not feel he could give an opinion. As to their scientific investigations he was inclined to place a higher value upon them than Mr. Burnell had done. Undoubtedly the first series of engineering publications he was acquainted with was the *Annales des Ponts-et-Chaussées*. He knew nothing in this country approaching them, and they formed a scientific literature of which any nation might be proud. There was one thing he could fully endorse,—that was the very high tone of honor amongst the engineers themselves. Their official pay was exceedingly small, but he believed a body of men with a higher professional tone did not exist. They did not aim at acquiring large fortunes, inasmuch as scientific men generally got in France that which they could only obtain here by becoming wealthy—they got high honor and social position. He happened to be at the New Year's levée of the Emperor, and he then inquired what was the precedence of the Corps des Ponts-et-Chaussées on such an occasion. He was told that the engineers took rank before colonels in the army, which was an evidence that their standing was fully acknowledged. But that was not the case in this country. There were brilliant exceptions, such as Faraday, and some others who, although they never sought for wealth, were still so distinguished that they could not be overlooked; but the great mass of professional men in this country were generally measured by the depth of their pockets. That was why they struggled to make money as quickly as possible, because money was the passport into society. The engineers of the Ponts-et-Chaussées had an entirely different tone of feeling. He knew many of them personally, and could speak of them in the highest terms. There was one remark in the paper about the architects which struck him. He understood Mr. Burnell to state that they were responsible for what is called the well-designing of their work for a period of thirty years. The period was a long one, but he thought it would be a wholesome thing if some such rule were acknowledged in this country. It was true, a man was responsible for what he did, because if he did not perform his duty, remedy might be had against him at law; but most persons would rather bear the loss than have recourse to the remedy. Then, again, the French system had its disadvantages. All those who practised as engineers in that country must have gone through the Ecole Polytechnique, and thus have been in some measure educated by the state, and so became naturally subject to state control. After all he felt that he could not say with certainty which system he preferred, there were so many elements involved in the question; but he would say he never went to France without coming back bitterly regretting that our present system did not, with our huge wealth and great commercial enterprise, produce results more worthy of the nation. If a few remarks on a subject, now becoming one of great interest in this country, were not out of place, he would say a few words as to the system of sub-ways formed in Paris, which

would serve as an illustration of the great care with which such works were carried out in France. His remarks would apply to Paris, Lyons, and other large towns. In the whole of the internal arrangements of those towns the French were beating us hollow: they were building better houses, forming better thoroughfares, putting up better lamp-posts, giving more light, and in all arrangements which affected the health of the community they were considerably in advance of this country. With regard to the main sewers of Paris, they were built in a style of magnificence which might be expected from a body who were not limited as to expense. He was one of those who thought the public works of large cities ought to be monumental, and not merely for the present generation. Surely it was an unworthy feeling that it was sufficient to carry out works which would last our own time, and to leave posterity to take care of itself. That was part of the money-greed of the age; and in that respect our public works contrasted unfavorably with those of Paris. The large intercepting sewers of the French capital were made with footways on each side. Those along the Rue Rivoli and the Boulevart-Sebastopol were so large that a full-sized man could walk on the footway on either side with his hat on, so ample were the dimensions of these sewers. They were kept in a state of cleanliness that was surprising. The entrances were made highly ornamental, which he thought superfluous; the steps which led down to them were wide enough for two persons to descend side by side. A railway ran along the edge of each of the footways of the large sewers, and below that was the invert forming the sewer proper. An exceedingly well-devised apparatus traveled up and down this railway, and mechanical scrapers were used by which the bottom of the sewer was constantly cleansed. The whole of the gas and water pipes were covered with a black varnish or bituminous compound; it was true there were leakages, but very few. The smaller branch sewers were most of them six feet in height, and on each side of them might be seen porcelain figures marking the house drains, with a small piece of white enamel about an inch square, which indicated that the proprietor of a particular house paid three francs annually for keeping his private drain clear. Every thing was carried out on the same scale. When they contrasted this with the way things were done in London, without entering into the political question, with which he had nothing to do, but looking simply at the material results as they were presented in the capital and large towns of France, it made us ashamed on looking at our own metropolis; and when one knew as much about the expenditure in London, as he did, as compared with the wealth, he must come to the conclusion that there was no town in France in which the inhabitants relatively to their means spent so little in keeping their city in decent and proper order as they did in London.

Mr. E. C. Tufnell remarked that the direction of the observations just made was altogether to depreciate English engineers below the French. He did not agree with that position, because there were many facts which went in contradiction of it. When the French desired to introduce any new engineering practice they almost invariably came to

this country for the assistance of engineers. When railways were first commenced in France they sent for Locke and other English engineers to carry them out. We were the first country to introduce suspension-bridges. The French sent a deputation to this country from the Ecole Polytechnique to inspect what we had done; they returned to France, issued an elaborate report, stating that our suspension-bridges were erected upon wrong principles, and devising (as they thought) better ones. They erected a suspension-bridge in Paris on their principles, but immediately upon its completion it tumbled down. Then, again, with regard to the Suez Canal, notwithstanding the predominance of French influence in Egypt, the Pacha sent to this country for engineering advice with regard to that project, and Mr. Hawkshaw went out to that country to make a professional report upon that great undertaking. Looking at these facts he could not at all reconcile them with the views expressed by Mr. Haywood.

Mr. Frederic Lawrence did not feel the same difficulty that Mr. Haywood felt, as to which system he preferred. As an Englishman he approved of a system which allowed talent to develop itself—by itself—rather than a centralized system which tended to cramp the energies and fetter the originality of those employed; and he thought if they compared the engineering works of this country with those of France, looking at the works themselves, they would see at once that our system was by far the best. Could the French engineers point to a Britannia Bridge? or if they had anything resembling it did they not copy it from us? Was not every large engineering work in France a copy or modification of something previously done in England? He was surprised to hear Mr. Haywood speak so approvingly of the sewers of Paris. It might be that the sewer along the Rue Rivoli was better than anything we have in London, but why was it better? Because the French engineers came over here and saw what we had done: they adopted what we had done well, and improved where they felt they could improve. It was, however, only recently that sewers had been introduced into Paris, and if that system were carried out all over the capital—as was stated regardless of expense—Paris might eventually be a better drained city than we could boast of. With regard to some of the details, he did not think Mr. Haywood, with whatever staff he could command, would care to have put upon him the task of fixing up the tablets he had spoken of, in the city sewers, indicating the numbers of the drains, and the sum paid by each private individual to have his drain kept clean. He had no hesitation in saying that, in his opinion, however admirable an institution the Corps of Ponts-et-Chaussées of France was, it bore no comparison with the engineers of England; because, when he looked round, he saw the names of engineers who had carried out works which the Corps of Ponts-et-Chaussées dared not have attempted. He therefore differed from Mr. Haywood, and had no hesitation in pronouncing the English engineers the first in the world; for, when any country was in difficulty, England was applied to for that valuable engineering counsel which could be obtained from no other country.

Mr. Haywood explained that his remarks had been misunderstood. He had dealt only with the broad features of the paper, and he had made no comparison between English and French engineers, as regarded their professional skill.

Mr. Lawrence understood Mr. Haywood to refer to the question of the two systems, and they must judge from the results whether one was better than the other.

Mr. Burnell said he had carefully guarded himself in what he had said against drawing comparisons between English and French engineers. Upon the question the relative merits of the two classes is was a singular fact that the French engineers had almost invariably consulted the English, and all the inventions that had altered the face of engineering science had arisen either in England or America. With regard to railways it was well known they originated in England, and the same thing occurred with reference to suspension-bridges. The merit of that form of bridge they knew belonged to Telford. Steamboats, the electric telegraph, and many other inventions, which had tended to advance civilization, were the results of the free-trade principles which prevailed in England, and of which he was a cordial supporter. The object of the paper was to call attention to the organization of the corps of the Ponts-et-Chaussées, which he thought, for carrying out works necessarily in the hands of the government, was superior to our system of appointing Government officers.

The Chairman, in proposing a vote of thanks to Mr. Burnell, said he thought they must all, to a great extent, participate in the feeling expressed by Mr. Haywood, on comparing the style of the public works in Paris with those of London; and with regard to the sewers, if anything was calculated to mitigate that feeling, it was, as far as he was personally concerned, in the visit he paid to the great brick tank at Barking, covering an extent of eleven acres, which for strength and excellence of execution could hardly be surpassed, and which showed what could be done in England in the way of first-rate brick-work. He was afraid they could hardly expect to settle the question this evening as to the comparative merits of the two plans, and he thought they must be content with supposing that each nation had settled down instinctively into the particular system which best suited its own peculiar constitution. He was sure they would pass a cordial vote of thanks to Mr. Burnell for his able paper.

The vote of thanks was then passed.

Spoked and Disk Wheels.

From the Lond. Practical Mechanic's Journal, April and May, 1864.

We have much pleasure in presenting to our readers the following interesting and important conclusions, which have been very elaborately wrought out by the celebrated French philosopher, M. Ad. Martin.

The large development of the railway system has caused the manufacture of the wheels of rolling stock to become one of immense importance.

The number of axles belonging to carriages and vans in France, at the 1st of January, 1862, was about 85,000, of which 3000 having disk wheels attached to them, represented a value of more than fifty millions of francs. The just combination of the several pieces of which they are composed, is one of the first elements of their security. The wearing out of the tyres is another most important consideration. It becomes thus a matter of great commercial interest that the wheels should be constructed after an improved type.

It is thought that all the requirements will be represented in the following terms:—

1st. To realize the greatest solidity amongst all the pieces.

2d. To offer as little resistance as possible to the action of the air.

3d. To be of light weight, and to offer as much resistance as possible to the binding energy of the tyre.

4th. To lift as little dust as possible when running.

Lastly. To produce at a minimum price an article of maximum durability.

Spoked wheels, which are employed for locomotive engines and rolling stock generally, are more costly, heavier, and offer a greater resistance, than those having plain disks, therefore they should be replaced by the latter. Another motive for employing these disk wheels results from their inferiority to extend a fire, should it occur in the carriages when traveling, and which spoked wheels to a large extent provoke.

The wheels of carriages and wagons being usually identical, there is no necessity to make a distinction between them. Their diameter outside the tyre being generally about 0·9144 metres to 1·05 metres, the first of which corresponds to about 3 English feet, and the second to 3 feet 7 inches nearly. It would therefore be of little benefit to go beyond this diameter, for if, on the one hand, the friction of the journal is slightly diminished, the weight of the wheel is increased, as well as the rolling friction, which, increasing as the diameter, destroys the advantage thus obtained.

The resistance that the atmosphere presents to the rolling action of wheels, is entirely independent of their diameter for a given rate of lineal travel, because in any case the circumferential velocity is equal, the number of revolutions corresponding with the diameter, accordingly as it is increased or diminished.

The general construction of spoked wheels being pretty well known, it is scarcely necessary to describe them in detail. They are usually made with straight spokes, the outer extremity of which are forged of a T form, these ends are then welded together in pairs, the manner in which the joint is made being one of the most important elements in the construction of spoked wheels; these are made secure at the centre, by a boss of iron cast over and between them. When the cast iron boss has become cooled, the outer rim formed by the T-shaped extremities is thoroughly welded, and made circular. There are many other ways of manufacturing spoked wheels for railway carriages and vans, and, indeed, they are now sometimes made entirely of wrought iron, in a

manner similar to that introduced by Mr. Gooch, on the Great Western Railway, for the wheels of locomotives.

Wheels with plain disks are much lighter and encounter much less resistance with the atmosphere during their revolution, they do not lift the dust, are not easily disturbed, and hold equally well upon the axles and in the tyres. Any person conversant with this subject, will thoroughly grant that a wheel composed of one piece of malleable or cast iron, in the form of disk, is much more solid and secure than one made up of an assemblage of pieces. It is, however, contended by some, that by the use of disk wheels, the tyres would soon become flattened, loose upon the wheels, and at the same time the wheels become loose on their axles. These prophesies, however, have not been realized in practice—they are contradicted daily, and, indeed, it appears that disk wheels hold better in their tyres than spoked ones have ever done. In either case, the chief element of secure fastening between the tyre and the wheel consists in the amount of strain produced by the tyre on the wheel, this adhesion being also increased by the introduction between them at intervals of bolts or rivets; in either, the bolts or rivets, their action is identical. It remains, therefore, to be seen what the actual value of this binding action amounts to, in shrinking the tyre over the wheel, the contraction of the tyre being usually brought about by first heating it, then passing it over the wheel. When thus placed in position, both are immersed in a cistern of cold water, where they are allowed to remain until the tyre is sufficiently cooled. It should also be stated that, prior to the tyres being heated, their internal diameter as either rolled or bored, is to be slightly less than the exterior diameter, of the wheel, in order that when expanded by heat, and again contracted by cold, they hold the wheel very firmly.

Let P = total pressure of the tyre upon the wheel.
p = pressure per unit of surface.
l = width of the tyre.
r = radius of the felly or rim of the wheel inside the tyre.
R = the co-efficient of elasticity of the iron, which, in the present case, is taken = (12 kil.) for each square metre.
s = the section of the tyre.

The total pressure of the tyre upon the wheel, and reciprocally, would be

$$2\pi rlp = \text{P} \quad . \quad . \quad (1)$$

In determining the tendency to produce diametral rupture, it was found that

$$p2rl = 2\text{sR} \quad . \quad . \quad (2)$$

In practice, s = 140 mill. × 55 mill. = 7700 mill. *en tenant compte du boudin*, when it would be—

$$pl2r = 2 \times 7700 \times 12 \text{ kil.} = 184800 \text{ kil.} \quad . \quad (3)$$

$$r = \frac{184800}{l2r} \quad . \quad . \quad (4)$$

making $2r = \cdot 94$ m. it would be

$$r = \frac{184800}{910 \times 140} = 1 \cdot 45 \text{ k.} \quad . \quad (5)$$

The pressure of the tyre on the wheel for each square millimetre of its surface

$$P = 2\pi rlp = 580{\cdot}052 \text{ kil.} \quad . \quad (6)$$

The expansion or stretching of tyre necessary to produce this pressure of 580·052 kilog. per square millimetre would be

$$A = \frac{P}{CS} \quad . \quad . \quad (7)$$

In which

A = the elongation the tyre has in metres, for every metre in length;
P = resistance of the wheel tending to stretch the tyre in killogrammes;
S = transverse section of the tyre in square millimetres;
C = co-efficient of the modulus of elasticity;
Acording to Poncelet, if P = 12 killog.;
C = to 20,000 kil.;

which would give

$$A = \frac{140 \text{ mill.} \times 55 \text{ mill.} \times 42 \text{ kil.}}{20{,}000 \times 7700.} = 0{\cdot}00060 \text{ m.} \quad (8)$$

This expansion would then be less than one millimetre for every metre in length of the tyre, and would not produce a pressure of 580052 kil.

It follows from the preceding investigations, that if the wheel itself were positively inflexible, the smallest diminution or increase of the diameter of the wheel under or over that of the interior of the tyre, would either convey a diminished or increased pressure upon the interior surface of the tyre, the latter of which represents the energy tending to burst it.

The question arises, Are disk wheels elastic? It is absolutely necessary to obtain exact knowledge on this point.

In spoked wheels, the total pressure of the tyre spreads itself over nine double spokes, that is eighteen single ones, therefore the energy of compression of the tyre that each one of these supports, will be expressed by

$$\frac{580052 \text{ kil.}}{18} = 32225 \text{ kilog.} \quad . \quad . \quad (9)$$

the dimensions of spokes being generally—

	m.
Length, . . .	0·290
Breadth, . . .	0·073
Thickness, . . .	0·045

According to M. Morin, the weight which each square centimetre of the section of the spoke would support, would be relatively to the preceding conditions = about 600 killogrammes, giving therefore

$$7\text{c}.5 \times 1\text{c}.5 \times 600 \text{ kilog.} = 6{\cdot}750 \text{ kilog.} \quad . \quad (9)$$

of contraction, so that the tyres could exercise upon each spoke a pressure of 32225 kilog., but exercising in reality only about 6750 kilog., that is to say, a fifth or thereabouts, which would reduce the entire energy exercised by the tyre upon the felly to $18 \times 6{\cdot}750$ kilog.=121,-

500 kilog., but it is supposed that the pressure which the tyre can effect, in the conditions about to be shown, that the centre of the wheel can support it, and if, as is seen, that the centre of the wheel can support a strain five times less, it will follow that the iron of the tyre will endure a tension which would correspond to the resistance offered by the wheel itself, and also it will be diminished in a proportion corresponding to the yielding or bending of the spokes at the time of putting on the tyre. It should be admitted, that in wheels the spokes of which are straight, and of a thickness of 15 millimetres, the tyre could not exercise an energy beyond 2·40 k. for each square millimetre of its own section, and that this would always be either above or below this amount in a proportion corresponding to the stiffness or flexibility of the spokes. It is then enough to state, that spoked wheels do not offer to the binding energy of the tyre a resistance sufficient to utilize the entire holding action it affords.

In the conditions of maximum pressure, the power that it would be necessary to exert to cause the tyre to slip on the wheel, would be 36,450 kilogs., taking 3 as the co-efficient of friction.

It becomes then advantageous to increase this binding energy by the insertion of five or six bolts or rivets, which for this purpose are usually of 0m. 035 diameter, and possessing a capability to resist shearing of about 50,000 kilogs.

The tyre influencing and exercising ordinarily, at the time of being put on the wheel, a pressure far greater than that which it (the wheel) can support, since with only a slight difference of the diameter there results an error which, by precaution, is always very great, in consequence of the bending of all, or of one part of the spokes, thereby producing an eccentricity of the journal with the circumference of the tyre.

Disk wheels on the contrary, are never out of shape, and present, at each point of the circumference, an equal resistance, which is greater than that of the wheels with spokes, and they have tyres and journals always concentric. The author has sought to determine the elasticity of a disk wheel by calculation; but precedents and formulæ are wanted to enable him to apply himself to the case, it has, however, been managed by proceeding experimentally, and much assistance has been obtained by the facilities and good feeling that he has met with at the works of the Orleans Railway Company.

From the following it will be seen how powerful the contraction of the tyre is, and how inferior the resistance of the centre of the wheel is to it; guided by these calculations, it has not been hesitated to put on the tyre of a disk wheel, *avec un ecart de diamètre* of 7 millimetres.

The tyre was 55 millimetres thick, with 127 wide. The centre of the disk wheel, furnished *par la Societé Providence*, had the following dimensions—

Internal diameter, . . .	0·920 m.
Breadth of the felly, . . .	0·082
Thickness of the rim of the felly, . .	0·013
Thickness of the side, near the felly, . .	0·013
Weight of the disk, . . .	130k.
Weight of the tyre, . . .	192k.

The wheel as well as the tyre was new; this last, made of iron and steel, had been furnished by M. Verdier.

In order to see whether or no the tyre could contract the disk wheel, three gauges were prepared, the first with points at a distance apart of 0m.65, the second 0m.40, and the third at 0m.30.

These measures, applied to the disk, equidistant from the centre on a diametral line, have been, as well as the diameters, set off with all possible care, and verified by many repetitions.

It was found on taking the tyre from the furnace the first time, that the dilatation was not sufficient; it was then put back in the furnace and brought to a red heat, then put over the wheel, where it was left until its temperature was reduced to that at which the tyres are tempered. Taken out of the water it was found sound, and the following statements are made concerning the body of the wheel:—

The diameter of the wheel had diminished 5 millimetres.
Two points, distant 0m.65, were drawn together 2·5 millimetres.
Those distant 0m.40, were drawn together 1·5 millimetres.
Those distant 0m.35, were drawn together ¾ millimetre.

These show, in the most complete manner, what it was wished to establish—the elasticity of the disk wheels.

As regards their resistance, it can be said that it is proportionate to the power of contraction of the tyre : : 2 : 5 since the tyre is enlarged 2 millimetres, whilst the wheel has diminished 5 millimetres; it is equally true that the tyre having stretched to its limit of elasticity, had attained a permanent lengthening which ought to be deducted from its total, to have the force *vive* of the tyre left free.

In order that a tyre, put on a disk wheel, might cease to be affected by it, it is necessary that it be lengthened; but the resistance of the iron being sensibly the same towards the traction as towards the crushing, the weight of a wagon has no bearing with the force that it would be necessary to exercise to produce that lengthening.

(To be Continued.)

On the Testing of Chain Cables. By Frederick Arthur Paget, Esq., C.E.

From the Journal of the Society of Arts, No. 598.

It is, no doubt, generally known that a select committee of the House of Commons is now considering a bill for the compulsory testing of the chain cables and achors of merchant vessels. This may be said to lend a passing interest to a question which, however, needs no chance help in calling for our attention.

Without entering into lengthy statistics, or calculating the number of times that the total length of all the chain cables in actual use would measure round the world, we should be scarcely mistaken in the supposition that in different parts of the globe there are, at this very moment, many hundreds of valuable lives, and thousands worth of property, in each case dependent upon a single link of the hundred fathoms that make up the length of an average chain cable; for there are situations in which a

ship is often placed wherein the cable must be literally the thread of life of the vessel. To the seamen of the present age, the iron cable, though of comparatively recent introduction, is a common every-day thing. Those of the last generation could remember the time when only hempen cables were in use. The naval men of that time were thus led to look upon chain cables as the most precious gift ever made in modern times to the mariner—to repeat the words of the late distinguished Captain Basil Hall.

Now, although we have been testing chain cables according to certain Admiralty regulations established ever since 1831, although the naval administrations of France, Russia, and other countries have exactly copied these regulations, and although Lloyds' have adopted the Admiralty test—which is somewhat more than the so-called "merchant" test—it is a remarkable fact that a difference of opinion with regard to almost every point connected with the use and testing of chain cables still exists amongst engineers and other men of science. This differing of doctors is very strikingly shown by the Blue Book report from the 1860 Select Committee of the House of Commons on anchors and chain cables for the merchant service. One witness states that 50 per cent. of the loss of life by shipwrecks are due to bad cables and anchors; another that very few wrecks occur through bad anchors and cables. One objects to the Navy proof as being too high; another as too low. One witness considers that the cross-stay does not add to the strength of the link; another that the cross-stay is a great improvement. In the same way, directly contrary opinions were elicited from different witnesses with regard to the duration of cables under wear, their re-testing, re-annealing, and other points. A similar want of agreement on these matters exists in France; and it would thus appear that several interesting engineering questions, connected with the strength, the testing, and the re-testing of chain cables, offer a fair field for a practical examination.

According to the Admiralty regulations, an iron chain cable has to consist of eight lengths, each 12½ fathoms long, including one swivel in the middle of every other length, and one joining shackle to each length. Neglecting the swivels and shackles, each link may be described as a cylinder, the axis of which is wound into a shape approximating to that of an ellipse. The width over all, or across the minor axis, is made 3½ diameters (full) of the cylindrical bar. The length over all, or across the major axis of the supposed ellipse, is six diameters. The cast iron stud across the minor axis is made 0·6 of a diameter in the centre, and one diameter at each of its ends. This stud not merely acts as a cross-stay, but also preserves the freedom of the joints, or what may be termed the mechanical flexibility of the cable. The weights are of course exactly fixed in the government tenders. The weight of, for instance, a one inch link stay-pin must not exceed 3½ ounces, and the weight, fixed by contract, of a hundred fathoms of cable, in eight lengths, including 4 swivels and 8 joining shackles, must not be exceeded by more than 1-20th part. The experience of many centuries has determined the sizes of hempen cables for ships of a given ton-

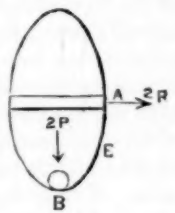
nage; and, the sizes of the hempen cable being thus given, it is easy to substitute a chain cable of the required strength. Mr. J. R. Napier has proposed a formula, according to which one-eighth of the cube root of load displacement would give the diameter of the chain cable usually employed by steamers of the present form. In the Admiralty comparative table, showing the weights and strengths of stud chains and hempen cables, there is a noticeable relation between the girths of the hempen cables and the diameters of the iron employed in chain cables. The number of inches of the circumferences of the hempen cables pretty nearly expresses in lines, or twelfths of an inch, the diameters of the iron cables of equal breaking strength. The material of the links is No. 3 rolled bar, and very good cable bolts generally cost from £1 to £2 above common bars. According to experiments by Telford, Hodgkinson, Mr. Edwin Clark, and Mr. Kirkaldy, and also according to numerous experiments at Woolwich, we may safely take the ultimate breaking strength of cable bars at 24 tons to the square inch, and their limit of elasticity, under a tensile stress, at 12 tons to the square inch. These bars would stand a pressure up to deformation of 18 tons to the square inch, and 15 tons pressure at the elastic limit. The ultimate tensile strength of a round bar of this iron would thus be nearly 19 tons. According to the evidence of the leading man of the test house at Woolwich, in 1860, this ultimate statical breaking strength is only occasionally exceeded, when it rises up to about 20 tons for one-inch round bar, or 25·33 tons per square inch. He also stated that a great number of experiments, made at Woolwich, showed the greatest breaking strength of one inch chain cables to be only 28·31 tons. Contrary to the popular assumption that a stud link should be, in the direction of its length, twice the strength of a single bar, this result would show a loss in strength of 28·75 per cent. According to the comparative table published by the Admiralty, the one-inch bolts should stand 21 tons 8 cwts., and the stud link therefrom, 34 tons 5 cwts. It also appears to have been assumed (for it could scarcely have been proved by experiment) that the strength of the cable bolt, and of the link therefrom, both increase almost exactly in the ratio of the diameter of the bars. Thus, the breaking of two-inch bolts is given as 21 tons 8 cwts., $\times 4 = 85$ tons 12 cwts., to which two tons are added; the strength of chain therefrom as 34t. 5 cwt. $\times 4 = 137$ tons, and the proof as $18 \times 4 = 72$ tons. It is, however, well known to engineers that, as a rule, a two-inch bar is not practically four times as strong as a one-inch bar of even exactly the same make and by the same maker, and that the strength becomes less and less as the bulk still further increases. The proportions adopted by the Admiralty appear, however, to compensate for this loss, and there is very nearly the same average ratio of breaking strength to diameter in all chains from five-eighths to two inches. But, even according to the Admiralty tables, there is a remarkable amount of strength lost in forming the iron into the link. This loss of strength was well known to Sir Samuel Brown, the introducer of chain cables. He thus patented, in 1817, the straight link

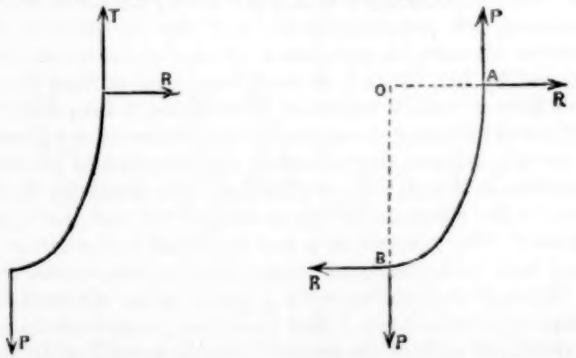
used in suspension bridges, and first applied it to the Brighton chain pier.

There are several reasons why a portion of the strength of the bar should be lost in forming it into a cable link. The principal causes are:—1st. The mechanical shape of the link; 2d. The crushing stress undergone by the insides of the crowns; 3d. The deterioration in strength of the iron through its being bent; 4th. The loss of strength at the welds.

In the first place, each link is, when the cable is pulled in the direction of its length, subjected to a transverse strain at each of its ends or crowns, and is somewhat in the condition of a curved beam loaded in the middle. An originally curved beam is, with regard to bending stress, in the same condition, at any cross-section at right angles to its neutral surface, as a straight beam under the same moment of flexure, The moment of flexure of one end of a common unstayed link can be expressed in inch-pounds by multiplying half the span, or half the distance in the clear, by the load in pounds. In the case of the stayed link, however, the moment of thrust of the cross-stay has to be subtracted from the moment of the bending force. The mechanically weakest part of any link is thus at the crowns.* Now, it is a curious fact, that all the writers on the strength of materials, from Professor Peter Barlow, Mr. Edwin Clark, and others, down to General Morin, in 1862,

* For the sake of simplicity, let us suppose the cross-sectional area of the link as infinitely small compared with its major and minor axes, and suppose it provided with a cross-stay. Let 2P denote the whole pulling force; 2R the thrust of the stay; T the tension at A. For the equilibrium of the quarter link, B, E, A, we have the forces P, R, T, and the forces at B arising from the left-hand quarter at A. From symmetry this must be horizontal (in the figure), and we must therefore have:—Force at B = R, and T = P. The moment of the bending force at B is therefore not P × OA, but only P × OA, — R × OB. On the other hand, when the link is on the point of breaking by opening at B, the tension will not be equal to the ultimate tension throughout the section at B, but only at the lowest point, and when this has given way a little, the tension, previously supported, is thrown on a fibre higher up, which then gives way, and so on. Hence the strength is less than if the tension were throughout the section as great as possible.





give the strength of a link furnished with a cross-stay to be equal to that of the iron of which the link is made.

In a mathematical sense, the contact between the links is only at a point, because it is a case of two cylinders touching each other at right angles. Under a load, this point will spread out to a surface of an area given by the amount of the load and by the compressibility of the iron. This surface will then probably increase, in the case of a one-inch cable under a load of nine tons, up to more than half a square inch. And thus at the ends, the softer and more ductile the iron, the sooner will it be worn away in practice, and the progressive deterioration caused by this crushing action will also be furthered by the friction.

An attempt to account for the reduction in strength through the bending of the cylindrical bar has next to be made. Wrought iron is known to be a crystallized body, belonging to the cubic system. Now, Mr. Mallet, in his important work "On the Physical Conditions involved in the construction of Artillery," has shown that these crystals are not grouped amorphously (or without distinct arrangement); but that they always take a certain determinate position. He has developed the law that "iron, whether in the state of cast or of wrought iron, has the principal axes of its integrant crystals arranged in the lines of least pressure within the mass," while exposed to pressure and heat in progress of manufacture. The principal axes of the crystals in a rolled bar would thus be set in a direction coincident with the length of the bar, and, from the property of malleability possessed by these metallic crystals, they would further take, under the pressure of the rolls, or the impact of the forging hammer, the longitudinal extension known as the "fibre" of wrought iron. Mr. Edwin Clark found that bars cut longitudinally and transversely to the fibre of the same plate of an excellent quality of iron, gave with the fibre a strength of from 19·66 to 20·2, and across the fibre a strength of only 16·7 tons to 16·93 tons. The ultimate elongation also of the plate in the line of the fibre was double as great as transverse to it. A great number of experiments by Mr. Kirkaldy gave somewhat similar results. He found that the difference averaged from 21·7 to 2·1 per cent., the mean difference in the whole being 9·8 per cent. in favor of the direction of the fibre. The respective ultimate elongations were also in almost the same rates as those found by Mr. Clarke. It would thus appear from these experiments, and from a consideration of Mr. Mallet's law, that both the elastic range and breaking strength of wrought iron of any given quality are, to a certain amount, dependent on the direction of the crystalline axes in relation to the strain; and further, the elasticity would be at a maximum in the direction of the principal axes of the crystals, or "line of fibre." The crystals in a bar subjected in the direction of its longitudinal axis to a tensile or compressive stress, would thus be in the most favorable condition with regard to its ultimate breaking strength and its elastic limit. But when, say, a red-hot bar is being bent, the principal axis of its crystals would, according to the law of cross-bending strains, arrange themselves above and below a neutral

axis in the direction of least pressure within the mass; the neutral axis would probably pass through the centre of gravity of the bar, the fibres on the concave side would be compressed, and those on the curved side would elongate in the ratio of their distances from the neutral axis. The hot iron itself would be, at any rate on the concave side, under a somewhat similar influence as when passing through the last pair of rolls, but the directions of least pressure, instead of being coincident with the length of the bar, would be at right angles to tangents to the neutral axis. Much of this is, of course, founded on several unproved assumptions, but it is at any rate evident that the molecular arrangement of the iron at the crown of the link is in the worst condition for resisting the tensile and compressive strains on each side of a neutral axis that make up the compound action of a transverse stress. The late Professor Daniel's process for unmasking the fracture and the arrangement of the fibre of wrought iron, by immersing the piece in dilute hydrochloric acid, would doubtless reveal a distortion of the crystals round a neutral axis.

It would thus appear that the crown of the link is its weakest part. This is, however, very far from being practically the case. Each link has of course to be welded up, and the weld is one of the sides, with a long scarf, in order to get a large welding surface. When we recollect that there are, in round numbers, 1800 links, and consequently 1800 welds, in a one-inch hundred fathom chain cable, and also that the efficiency of the cable depends on each individual link, the paramount importance of the welds is obvious. In nine cases out of ten, while in use and while being tested, the links are found to give way at the sides. Breakages would, *cæteris paribus*, have a tendency to occur at the welds with good iron but bad workmanship, and in the iron, and not in the weld, if good workmanship but bad iron were employed. The uncertainty of welds is in any case well known to practical men. Mr. Kirkaldy has made some eighteen experiments on the relative strengths of welded joints in wrought iron. Some of these welds were made by a chain maker. Only six of the specimens broke solid away from the weld, and in every case there was a loss of ultimate breaking strength averaged from 2·6 to 43 per cent., the mean being nearly 20 per cent. As with almost every thing else belonging to the subject of chain cables one of the witnesses before the Committee of 1860 raised the question whether the position at one of the sides was the best for the weld. Mr. Smale, of Woolwich, proposed to weld the link at the crown, as there would thus be more room for the smith, and any bad weld would be less hidden by the cross-stay. The crown is, however, as we have seen *ab initio*, the weakest part of the link. Besides, if a weld at the side gave way, the other half might catch and save the cable; at the same time, however, a sudden giving way at the weld would cause an instantaneous distortion and probable rupture of the opposite side, as the sudden "run" of the cable would act with an impulsive force. In fact, when iron cables were first introduced, the welds were made at the crown, but the plan had to be given up. It is clear enough that there are, *cæteris paribus*, three weak places in a link where any effects

of stress would first show themselves,—the two crowns and the weld at the side.

We thus see what a powerful element of uncertainty is brought by the uncertainties of workmanship into such an apparently simple thing as a chain cable. When, however, we remember that the very best wrought iron of commerce is, to use the words of the well-known metallurgist Saint Claire Deville, but a metallic sponge, like platinum, the pores of which have been simply closed up by pressure or percussion; that, in one word, ordinary wrought iron has never as wrought iron, been fused, it will be seen that the uncertainties qualifying the material itself are still greater. Mr. Mallet thus found that while the original hammered slab of a very large forged mass had a breaking strength of 24 tons to the square inch, it fell progressively to 17 and 16 tons at the different places of the mass, down to even as low as 6½ tons in some parts. Unless this iron had been burnt its tenacity could doubtless have been restored, and if drawn into wire, its breaking weight might have been increased to perhaps 90 tons to the square inch—at least before annealing. An average of 188 experiments, made by Mr. Kirkaldy on rolled bars, gave a maximum breaking strength of 30¾ tons, and a minimum of nearly 20 tons to the square inch. These influences of the manufacture merely on the quality of wrought iron are almost independent of the chemical constitution of any individual bar. For instance, until it be proved to the contrary, there are many reasons for the general belief that the cold shortness of wrought iron is due to the presence of silicon and carbon; and its hot shortness to that of sulphur. A fractional per centage of copper also makes wrought iron hot short. In truth, there are probably no two bars or parts of a bar of an exactly similar chemical composition, or in an exactly similar state of molecular aggregation, and therefore of an exactly similar breaking strength or elastic limit. Even these are only a few of the elements of uncertainty in structural materials. But when we further take into account the varied strains of extension, compression, distortion, twisting, and bending, to which mechanical structures are more or less subject; that the work done by a gradually applied load is doubled if this load be applied suddenly; that the impulsive strain of a moving load is generally more or less intensified by vibration; and that the varied shapes and arrangements intended to receive these strains must be often as much fixed by financial as by scientific considerations, then the reason that the best engineering practice makes the ultimate strength of a wrought iron structure from four to six times the working load must be even popularly evident. But these factors of safety are not sufficient. The structure must be tested as searchingly, and as far as is consistent with safety—as far as is possible without injuring the material and its relation to the structure. In our case this limit is, in the main, given by the limit of elasticity of wrought iron under extension, as this limit is less for wrought iron than that of compression. It is also self-evident that the mode of testing adopted ought to approximate as nearly as practicable with the kind of stress the object is intended to undergo in practice. It is also evident that if circumstances

allow us to exceed this limit, if, in fact, we can push the test as far as the breaking strength of a portion, or of an individual piece of the object, we shall obtain the safest amount of information about its qualities. In this way guns and plates are both tested to destruction. In order to test the probable performance of rails under a moving load, a certain number, taken from a lot, are broken by a falling weight, the distance between the supports and the height of fall being fixed by contract. The French test their railway carriage axles in a somewhat similar manner. There is no test so good as a blow for detecting a false weld. In Sweden they do not confine themselves to the usual gunpowder proof for gun barrels, but two or three sharp taps with a hammer are given along the breach, which have an immediate effect on a bad weld. All the whipple chains for the carriages of the Royal Swedish Artillery are tested by letting the loose end fall from a height double the length of the chain, after being attached to a weight. The anchors for the French imperial marine are tested by being dropped from a determined height for each size. The axles for the carriages of the Messageries Générales and the Imperial Artillery are tested by the impact of a falling weight. All the swords and sabres for the army are tested by striking the blades on a block of wood. When we advance from details to considerable structures, we are, of course, obliged to very carefully confine ourselves within the limits of elasticity. After loading a railway bridge with the greatest passive, or perhaps impulsive load that would ever come upon it in practice, the deflection, and the permanent set, if any, are both carefully noted. As a boiler is subjected in practice to a complex train of mechanical and chemical forces that are always striving to break their bounds, its ultimate strength is made from six to eight times the working stress, and it ought to be periodically tested to half its working pressure. Its extension under this pressure is sometimes—and should always be,—measured by the volume of water that is pumped in by pressure after the boiler has been filled; while the permanent set is determined by the difference between the volume pressed out by the contraction of the boiler when the pressure is withdrawn, and the volume of the water that remains in the boiler after the test—allowance being of course made for any slight leakages and sweating at the joints. In first-class locomotive works the deflexion and permanent set of the steel springs are always tested in an apparatus for the purpose. It may here be remarked that, although the designs of all the successful wrought iron structures ever built have been based on the assumption of a limit of elasticity, nevertheless the relation of the permanent set of wrought iron to its ultimate resistance under a given load, is still a subject of some discussion. We have, on the one hand, the testimony of Professor Eaton Hodgkinson, who says that "the maxim of loading bodies within the elastic limit has no foundation in nature;" and, on the other hand, some appear to believe that iron is even improved by breaking it under, at least, a tensile stress. Mr. Hodgkinson found that a rod, 10 feet long and of one square inch section, took a permanent set of 0·0005 of an inch under a static load of less than $1\frac{3}{4}$ tons. Mr. Edwin Clark obtained very similar re-

sults. Such a microscopic set, however, could be referred to the elongation caused by the heat generated by the internal friction of the particles, or to the probable fact that these bars were also new; and it is conceivable that they might have taken a slight permanent set, just as new rope takes a permanent set, without injury, when the strain is first applied. There are, indeed, very few forms of wrought iron in which its internal particles are not, *ab initio*, subject to some mutual strain. At any rate, these elongations were very slight, and increased uniformly up to tensions varying from about 10 to 15 or 16 tons on the square inch. Beyond these strains the bars elongated in an irregular manner until they at last broke. At the same time, as Dr. Rankine remarks, the demonstration by Mr. E. Hodgkinson that a set is produced by a strain much less than what would injure the specimen, renders the determination of the proof-strength a matter of some obscurity; but Dr. Rankine points out that the best test now known is the not producing an increasing set by the repeated application of a load. Some years ago, Mr. Loyd, of Woolwich, made certain experiments which have been cited as proving that a breaking strain does not injure iron, even when this strain is four times repeated; or rather, that after breaking a bar into, say, two pieces, the two pieces are thereby made stronger. In, for instance, experiment 2, the 1⅝ bar marked C was found to break with 33¾ tons, with a stretch of 9¼ inches in 54 inches; a piece of this bar then broke at 35¼ tons, with a stretch of only a quarter of an inch in 36 inches; another piece of the bar, 24 inches long, was broken at 37 tons, with a stretch of one inch; and at the fourth and last breakage was found to give way at 38¾ tons, but without any stretch at all. Results of a similar delusive kind, obtained by Professor Walter Johnson, were communicated by him to the United States government in 1845. He found that by heating a bar to a temperature of 400° Fahrenheit (or the temperature of steam at about 250 lbs. pressure), and stretching it permanently for about 6½ per cent. of its length, it, on being broken, gave an ultimate breaking strength about 20 per cent. higher than a portion of the bar that had not been heated and stretched. He therefore supposed that, to use his own words, "the value of useful purposes, added to a bar of iron by thermo-tension, when the increase of both length and tenacity is taken into account, may be safely set down at 26 per cent. of its original value. It sometimes exceeds 30 per cent. On a single cable, 100 fathoms long, made of iron 2½ inches in diameter, weighing about 15 tons, and attached to a line-of-battle ship, the gain, in true commercial value, would not probably fall short of 600 dollars." A machine was made by the American government, in order that the Professor might apply his principle of "thermo-tension" directly to chain cables, but as nothing else ever appears to have been heard about the matter, we have thus, as usual, lost another of the lessons always taught by scientific accounts of scientific failures. The pitch chains of the old engines of the *Great Britain* are stated, by Mr. Guppy, to have been stretched one-eighth of an inch while at a low red heat. This was, no doubt, an excellent method for testing the soundness of the work. Captain Blakely also

stretches the hoops that are shrunk on his guns. This is done on a maundrel, and while the rings are at a red heat; but it is stated that only one-sixth of the breaking strain of the cold metal is applied. The action of the maundrel also probably re-arranges the crystalline aggregation which had been disturbed by bending the ring from a straight slab. The red heat of iron is only visible in daylight at a temperature of 1077 degrees Fahr., and the heat used by Professor Johnson was only from 400 to 500 degrees. But the "gain of length"—the permanent set, in fact—of from 5 to nearly 7 per cent., sufficiently shows that the bars had either been broken or were close upon fracture. His results, in fact, merely anticipated those of Mr. Loyd. The breaking strength of his bars was doubtless increased, but with a proportionate diminution, perhaps, indeed, a complete destruction, of their elasticity. They were rendered harder, for what is the hardness of a body but the resistance of its particles to any temporary re-adjustment. The longitudinal elongation was accompanied by a lateral contraction of the cross-sectional area that would reach its culminating point at the part where fracture happened to take place. Exactly the same argument, founded on similar experiments on cables themselves, was used before the 1860 Committee, in order to prove that cables are not injured by a breaking strain; but a mere statement of the progressive diminution of the elongations would have detected the fallacy.

(To be Continued.)

MECHANICS, PHYSICS, AND CHEMISTRY.

Proceedings of the Association for the Prevention of Steam Boiler Explosions, Manchester.

[Report of Chief Engineer, January 5, 1864.]

From the Lond. Mechanic's Magazine, Feb., 1864.

During the past month there have been examined 466 engines and 730 boilers. Of the latter, 11 have been examined internally, 75 thoroughly, and 645 externally, in addition to which 11 of these boilers have been tested by hydraulic pressure. The following defects have been found in the boilers examined:—Fracture, 13 (1 dangerous); corrosion, 29 (5 dangerous); safety-valves out of order, 2; water-gauges ditto, 11 (1 dangerous); pressure-gauges ditto, 24; feed-apparatus ditto, 2; blow-out apparatus ditto, 51; fusible plugs ditto, 8; furnaces out of shape, 2 (1 dangerous); over-pressure 6; deficiency of water, 1 (dangerous); blistered plates 1. Total 150 (9 dangerous). Boilers without glass water-gauges 7; without pressure-gauges, 7; without blow-out apparatus, 31; without back-pressure valves, 36.

Instances of serious as well as of dangerous corrosion, both internal and external, continue to be met with, which shows the importance of all the plates of a boiler being regularly submitted to careful examination.

Among other cases of corrosion which have been met with during the past month, the details of the following may be briefly given. In one instance, a boiler lately put under the charge of the Association, was found on the removal of a portion of the mid-feather, at the first examination, to be so eaten away by corrosion, that the merest touch made a hole through the plates, and several had to be renewed before the boiler was fit for work. In a second instance, the boiler—also recently enrolled—proved to be nearly corroded through in several places in consequence of leakage from joints which were bolted instead of being riveted. In a third instance, a two-flued boiler, 7 ft. in diameter, was found to be so corroded that the plates in one of the external flues were reduced to about one-eighth of an inch in thickness, which is a most serious reduction for a boiler of such dimensions. In a fourth case, the under side of the furnace flue of a Cornish boiler was so weakened by internal corrosion that the inspector made a hole through the plates on sounding them externally, *i.e.*, from the inside of the flue. This being a single-flued boiler, and the corrosion having taken place at the bottom of the furnace tube, the injury could not be seen, even on an internal inspection, and therefore was liable to escape detection; this is not the case with a "Lancashire" boiler, in which the bottom as well as the top surface of the furnace tubes are accessible to examination. In a fifth case, a boiler, which had not been thoroughly examined for upwards of two years, was found to be most seriously affected internally by the action of corrosive water. Many of the rivet heads were half eaten off, and presented a spongy appearance, the plates were extensively honey-combed, some of the indentations being as much as one-eighth of an inch deep, and others on the furnace crowns a quarter of an inch; while the longitudinal stays were eaten into holes and in some parts reduced to half their original strength, as well as the cotters seriously affected; in addition to which one-half of each of the internal flue angle irons was eaten away, some of them being brought up almost to a feather edge. It is scarcely three years since the boiler was new; while continuance of such service without attention would soon have rendered it unfit for work altogether. In cases of acidity in the feed water, simply soda is found most effective, and had been recommended by the Association in the present instance, the corrosive action being suspected from the appearance of the water in the glass gauge, though its effect on the boiler remained unknown till the thorough examination was made. In a sixth case, grooving or furrowing action proved to have worked its way through the solid plate within the thickness of a sheet of paper.

These cases which are not peculiar, and were all met with during the last month's inspections, will suffice to show the importance of the members affording an opportunity for each of their boilers to be "thoroughly" examined at least once every year. In order, however, that these examinations should be satisfactory, it is imperatively necessary that the flues should be well swept and the plates brushed. When several inches of soot remain in the flues, and the plates are at the same time coated, it is impossible for the inspector either to do himself or the examination of the boiler justice, and simple as the pre-

paration of boilers for "thorough" examinations may appear, yet it has so important an influence on the value of the inspection, and at the same time is so frequently neglected, that the attention of the members is once more called to the subject.

Advantage has been taken of the past Christmas week to make as many "thorough" examinations as possible, and it may be stated that it is important, in order to prevent disappointment, that those members who desire to avail themselves in this way of these general holiday times, such as Easter and Whitweek, &c., should make early request for inspection, since it is impossible at a few hours notice—as is too frequently expected—to alter arrangements that have previously been made for days.

Explosions.—No. 18 Explosions occurred to one of a pair of marine boilers, not under the charge of the Association, and working on board a tug-boat. They were not multitubular but flued boilers, each having two furnaces which united in a single return flue and then passed off to the uptake.

The explosion was caused by the collapse of the outer or wing furnace crown, of the starboard boiler. The furnace crowns, though not entirely semicircular, were arched elliptically, being made of plates of seven-sixteenths of an inch in thickness, and worked at a blowing-off pressure of 28 lbs. The crown of the wing furnace came down till it rested on the bars and rent at a transverse seam of rivets immediately over the fire, the adjoining furnace being also slightly depressed but not rent and no other part of the boiler damaged. The fireman, who was the only one below deck at the time, was scalded to death.

This boiler was not personally examined, but the collapse is reported to have been due to shortness of water, and there does not appear on consideration of all the circumstances, to be any reason to differ from this conclusion.

No. 26 Explosion.—The boiler in question, which was not under the inspection of this Association, was of plain cylindrical egg-ended construction, and externally fired.

Its length was 30 ft., its diameter 6 ft., and thickness of the plate one-half of an inch. The longitudinal seams of rivets were arranged so as to break joint, the boiler being well made throughout and new about nine months previous to the explosion, while none of the plates appeared to be weakened by corrosion neither was there any evidence of leakage at the seams. The boiler was liberally supplied with the fittings usually to its class, having duplicate floats with alarm whistle, duplicate lever weighted safety-valves, perfectly open and a feed back pressure-valve, &c., while the blowing-off pressure was 55lbs., which was by no means excessive for such a boiler.

On examination of the fragments of the shell, it was found to have separated into four distinct pieces, both of the dome ends with one or two rings of plate, having torn away at the transverse seams, while the remainder of the cylindrical portion of the shell had rent longitudinally, and also separated into two pieces.

The flight of the dome ends, which had taken place in opposite directions, had been productive of considerable injury; the one at the firing end flying out of the works, crossing an adjoining street and falling against a dwelling-house on the opposite side, and thereby very seriously injuring one of its inmates, but fortunately not fatally so; while the other dome end of the boiler, skated as it were along the ground, sweeping down a great deal of building in its course, so that a considerable portion of the works was reduced to a heap of ruins, and the *debris* scattered in every direction.

In stating the cause of the explosion, it should be pointed out, that the boiler though externally fired was set upon a mid-feather, up one side of which the flames passed and then returned on the other; while the flues were carried up to a level with the centre of the boiler, to which height, therefore, the flames could act upon it. The flame passed on the right hand side of the mid-feather in the first instance, and it was just at the top of this flue and a little beyond the bridge, where the flame would be in full force, that the primary rent, which was a longitudinal one, appeared to have occurred in the shell of the boiler. This naturally awoke the suspicion that the water level had been allowed to fall below the top of the flue, and this view was strengthened by an examination of the plates, which both from their color, as well as from the character of the rupture, appeared to have been overheated; while in addition, there were water lines plainly marked on the inside of the boiler considerably below the top of the flues, so that it appeared highly probable that the water had not been allowed to run short for the first time on the day of explosion. The boiler attendant acknowledged that he had not tested the floats to see if they were free, for about four hours previous to the explosion, so that there seems no room to doubt, that it was due to shortness of water; which, however, it may be added, is the only instance of the kind that has been met with during the past year, in those explosions personally investigated by this Association.

No. 27 Explosion was the result of the old age and neglected condition of the boiler in question, which was not under the charge of this Association, while fortunately no one was killed by the explosion, and only one person injured.

The boiler was but small and of plain cylindrical egg-ended construction externally fired, being only 12 feet long and four feet in diameter; the plates varying in thickness, some being seven-sixteenths and others three-eighths of an inch. The pressure was stated to have been 25 lbs. per square inch, but there had been no steam pressure-gauge, and the fittings were altogether of a meagre description. The boiler was plated so that the longitudinal seams did not break joint, but were in line or nearly so, some of them falling immediately over the fire, and it was at one of these which had been clumsily patched, that the boiler ruptured, the rent extending from one end to the other, and the shell completely opening out.

Its flight was somewhat remarkable; it took down the chimney in its course, flew over a row of dwelling-houses, as well as a public road

and a canal, landing finally in the bed of a river, immediately under one of the arches of a railway bridge which ran across it.

As to the cause of the rupture, the boiler was at least 25 years old had been working unroofed, and thus exposed to the action of the weather, and was found upon examination to be seriously corroded both inside and outside, having been patched in several places in consequence. This corrosion had eaten off the heads of many of the rivets, and seriously reduced the thickness of the plates, especially at the longitudinal seam over the fire at which the rupture had taken place; so that this explosion is simply due to the dilapidated condition in which the boiler had been allowed to fall through old age and neglect.

No. 34 Explosion.—The boiler in question was not under the inspection of this Association. It was internally fired and of single flue or Cornish construction, its length being 23 feet, the diameter of the shell 5 feet 6 inches, that of the furnace 3 feet for a length of 7 feet, and in the flue 2 feet 6 inches, while the thickness of the plates was five-sixtenths of an inch, and the pressure at which the steam blew off 40 lbs.

The furnace flue, though reported to have been amply covered with water at the time, collapsed immediately over the fire at about 2 feet 6 inches from the front, which admits of no surprise considering the diameter of the flue, the lightness of the plates and the pressure of the steam, so that this explosion can only be regarded as one of those already so numerous, which might have been prevented by simply strengthening the furnace flue with any one of the inexpensive means frequently called attention to in these reports, viz: flanged seams or hoops, whether of T-iron, angle or other suitable section, as well as by the adoption of other approved methods.

The boiler was not moved from its seat by the explosion, and the glass water-gauge and steam pressure-gauge remained uninjured, but owing to the scalding effects of the steam and hot water which escaped from the rent, as well as to the flight of the furnace mountings and other fragments which were shot away, three persons were killed and two others seriously injured.

No. 40 Explosion.—This is the second instance which has occurred within the last few months, of a new boiler exploding at the makers yard while being tested with steam, and which might have been prevented by the simple use of the hydraulic test. Two men were killed and seven others injured by the explosion of this boiler, which was not under the inspection of this Association.

The boiler which was intended for a tug-boat was of the flued class, internally fired, having two furnaces, and being very similar to that described under explosion No. 18, while it had a large steam dome on the top through which the flue passed on its way to the chimney.

The shell rent along the top longitudinally, and was completely opened out and laid flat upon the ground, in which position it, as well as the flues which were uninjured, remained unmoved, while the steam dome was torn completely away and shot upwards, being blown over some high buildings into an adjoining yard.

The fact of the steam dome being torn off, and the rent starting

from it on the top of the shell, as it appears to have done, clearly shows the dangerously weakening effect that these large openings have upon boilers, so that there can be no doubt that this boiler was of defective construction, and that this fact might have been detected in time, the explosion prevented, and the lives saved, had the hydraulic test been adopted.

No. 41 Explosion, which resulted in the death of both of the engineer and fireman, occurred to a boiler not under the charge of this Association, and at too great a distance from Manchester to be personally investigated, while considerable difficulty has been experienced in gaining authentic information with regard to it.

It appears that the boiler was one of two of Cornish construction connected together and working side by side, and that the internal furnace flue collapsed from end to end, rending across three of the transverse seams of rivets.

The collapse was attributed to the boiler having been allowed to run short of water, the jury giving it as their opinion that "The deceased men died from scalds caused by the bursting of a tube in the boiler, arising from want of water."

The explosion occurred at about six o'clock in the morning, the boiler being worked night and day, and it appears that the night fireman had been drinking and thus neglected his duty.

No. 42 Explosion resulted in the death of four persons as well as injury to three others. The cause of the explosion of this boiler, which was not under the care of this Association, has been made the subject of an official scientific investigation, and as soon as a copy of the report is received, which is promised shortly, the particulars will be communicated to the members.

It may however be stated in the meantime, that the explosion has been generally attributed to shortness of water, but it appears from information kindly afforded by an engineer, who made an examination shortly after the explosion, that the furnace flue of the boiler—which was a Cornish one—was seriously out of shape and unfit for work; so that the explosion seems to have resulted simply from weakness of the flue and not from want of water.

No. 43 Explosion, from which one person was killed and four others injured, was in many respects very similar to No. 41, being due to collapse, which extended throughout the entire length of the furnace tube of an internally-fired single-flued Cornish boiler, which was not under the charge of this Association, while the distance at which the explosion occurred precluded a personal investigation. It appears, however, from a report obtained that the length of the boiler was 20 feet 6 inches, its diameter 5 feet 6 inches in the shell, and 3 feet 1 inch in the furnace flue, which was made of plates three-eighths of an inch in thickness. Only one of the two safety-valves with which the boiler was fitted was in working order, while the ordinary pressure of the steam was stated to have been 49 lbs. on the square inch, but what it may have been at the time of the explosion was not actually known.

A furnace flue of such dimensions as the above might when new, if

of good materials and well put together, as well as of true circular shape, work for a time at a pressure of 40 lbs. per square inch, but—taking into account imperfections in workmanship in the first instance, the effect of several years, wear and tear, and the frequent excess of the actual over the supposed pressure of steam, which is frequently found to exist through errors in safety-valves and steam pressure-gauges—it certainly is not prudent to work within so narrow a margin of danger, especially when safety could be insured simply by hooping the flues; and although the collapse was again in this instance attributed to shortness of water, it is thought to be rather to the neglect of the above precaution, that the explosion is really due.

During the past month three explosions have occurred, from which five persons have been killed and four others injured; not one of the boilers was under the inspection of this Association. One of these has been personally examined subsequently to its explosion, while distance prevented this being done in the other cases. The following is the monthly tabular statement:—

Tabular Statement of Explosions, from November 21, 1863, *to December* 31, 1863, *Inclusive.*

Progressive No. for 1863.	Date.	General Description of Boiler.	Persons killed.	Persons injured.	Total.
44	Nov. .	Double furnace "breeches" boiler Internally fired, . . .	0	0	0
45	Nov. 23.	Cylindrical egg-ended, Externally fired, . .	1	4	5
46	Nov. 27.	Ordinary single flue or "Cornish" Internally fired, . . .	4	0	4
		Total, ...	5	4	9

No 44 Explosion occurred to a boiler not under the inspection of this Association, and at too great a distance from Manchester to admit of a personal investigation being made, but the following particulars have been kindly furnished by an engineer who examined the boiler shortly after the explosion took place.

It was a breeches boiler, internally fired, having two furnaces running into one flue. Its length was 15 feet and diameter 6 feet, while the ordinary working pressure was 60 lbs.

The boiler rent along the bottom longitudinally from one end to the other, just as if it had been set upon a mid-feather, and the plates thereby corroded away; but it proved on inquiry to have been but six months old, and set upon two side walls, so that a longitudinal rent under these circumstances is peculiar. The cause of the rupture appears to have been as follows:—A heavy cake of deposit, chiefly of salt, and several inches thick, had through neglect been allowed to form inside the boiler along the bottom. This, under ordinary circumstances, would have not been attended with any dangerous consequences in an internally-fired boiler, since the temperature in the external flues

is not usually sufficiently high; but in this case the conditions were peculiar; the boiler as will be seen by the dimensions given above, was an unusually short one, at the same time that the draft was good, and the furnaces heavily fired. Since, therefore, the course of the flame passed under the bottom of the boiler immediately after leaving the internal flue, it is not difficult to understand how the plates at the bottom of the shell, which were heavily coated internally with sediment, became overheated and then rent. Surface blowing out would, in all probability, have prevented this explosion.

It will be seen that the circumstances of this boiler were such that it was virtually fired internally and externally at the same time, and it therefore affords an apt opportunity for comparing the safety of the two systems of firing, and it will be noted that the explosion was not due to that portion of the boiler which was fired internally, but to that which was fired externally.

It was not only fortunate for the two men standing in the stoke hole at the time of the explosion, but it is also an interesting engineering fact, that sufficient warning was given them to make their escape, one of them hearing the rush of the water directly the rent commenced, and giving the alarm, while the boiler lifted from its seat and flew through the roof an instant after. This fact is an important one, since it shows, that in this instance, at all events, no explosion like that of gunpowder took place, though such is generally supposed to be the case, but that the result was due simply to the pressure of the steam acting according to its ordinary laws, and to the quite rending of the plate.

No. 45 Explosion.—This boiler was not under the inspection of this Association, neither was it personally examined after its explosion, which, however, is reported to have been the result of almost unparalleled recklessness.

The boiler was a stationary one of plain cylindrical egg-ended construction, and externally fired, being 16 feet long, 3 feet 6 inches in diameter, and made of plates three-eighths of an inch in thickness. It was employed for agricultural purposes, and under the charge of a fireman as well as of a superentending foreman or farm bailiff. The engine which it drove had lately undergone repair, and steam was being got up for a fresh start, when the foreman, in spite of remonstrance, persisted in keeping the safety-valve down by sitting on the lever, the carpenter being engaged in the meantime, by the foreman's orders, in cutting a wooden prop to jam the valve down. Before, however, the prop could be obtained, the boiler exploded, instantly killing the foreman and severely injuring the engine-man and the other persons present.

The boiler was completely severed into two parts, one portion being blown to a considerable height, and thrown to a distance of 150 feet, the rent passing through the manhole and the greater part of the second transverse seam of rivets from the front end, which was situated immediately over the fire. The position of the safety-valve on the boiler was such, that the man whilst sitting upon the lever was immediately

over the manhole, and both he and the cover, were blown by the explosion in the same direction, and picked up at a considerable distance from the original seat of the boiler.

With regard to the cause of the explosion, there is no ground for concluding that the steam, although the valve was held down, had time to attain to the bursting pressure of a boiler of such dimensions as this one was, supposing it to have been in good condition, and there seems, therefore, reason to think that the boiler had been somewhat weakened by the twelve years service it had seen, although there can be no question that the immediate occasion of the explosion was excessive pressure caused by recklessness such as these reports have not previously had to record.

No. 46 Explosion, which resulted in the loss of four lives, occurred to the right hand boiler of a series of three working side by side and connected together. These boilers were not under the inspection of this Association.

The one which exploded, as well as that next to it, was of Cornish construction, being internally fired, and having a single flue running through it, which diminished in diameter behind the fire-bridge. The length of the boiler was 26 feet 3 inches, the diameter of the shell, 8 ft. 3 ins. that of the furnace tube 5 feet for a length of 8 feet, and of the flue, 2 feet 9 inches, while the thickness of the plates throughout was scarcely three-eighths of an inch, being especially bare in the furnace.

The boiler had been fitted with a feed back pressure-valve, a brass blow-out tap, and a glass water-gauge, as well as with an ordinary lever safety-valve and a steam pressure-gauge common to boilers No. 2 and No. 3. The pressure at which the safety-valve was stated to have blown off was 40 lbs. by the gauge, but on calculation it appeared that this should be taken as minimum, and that with the steam blowing-off freely the pressure would no doubt rise as high as 45 pounds, if not 50 pounds.

On examining the boiler, the furnace tube was found to have collapsed immediately over the fire, and rent at one of the transverse seams as well as at its attachment to the front end plate. The collapse had not extended into the flue behind the fire-bridge, or affected the conical junction piece between the flue and the furnace tube.

As to the cause of the collapse there cannot be a doubt. No flue 5 feet in diameter, made of plates barely three-eighths of an inch in thickness, can be safely subjected to a pressure of 40 lbs. or 50 lbs. on the square inch, as this one was; and hence the explosion. The boiler was an old one, and though not materially weakened by corrosion, had evidently been made at a time when the use of high pressure steam was very little understood. The cylindrical portion of the shell was stayed transversely with horizontal bolts both above and below the furnace tube, as well as with others running vertically on each side of it, while the flat ends were stiffened with diagonal stay bolts. No attempt, however, was made to strengthen the large furnace tube, though the weakest part of the whole boiler, while the diagonal rods introduced, are very inferior to the gusset stays now in use, and the

horizontal and vertical ones afford but little or no support to the cylindrical portion of the shell.

The personal injuries due to the explosion arose from scalding, while the effects on the surrounding property were but trifling, which is frequently found to be the case when explosion results from collapse of the flue of an internally fired boiler. In this instance the boiler was not stirred from its original position, nor the joints of the steam pipes broken, and in order to be in a position to work on with the other boilers, it was only necessary to screw down the junction valve. The principal injury to the surrounding property resulted from the flight of the furnace mountings, which in this case were unnecessarily heavy, and included a facing of brick-work, which is always objectionable in such a position. It will thus be seen that in the case of collapse, the weight of the furnace mouth-piece becomes a source of danger, and the fact of damage frequently arising from this cause, and in some cases loss of life, is an argument for the adoption of light wrought iron furnace mountings, which are far superior as a mechanical arrangement.

The insufficiency of this boiler for the pressure at which it was worked could not have escaped detection either on competent inspection, or the application of the hydraulic test.

There can be little question that many explosions occur in different parts of the country of which no report is obtained by this Association, but it will be seen from the tabular statement above that 46 came under its notice during the year 1863. The number of boilers which exploded, however, amounts to 50, since an entire series of 5 working side by side was included in one explosion; while during the year there were 76 persons killed, and 77 others injured.

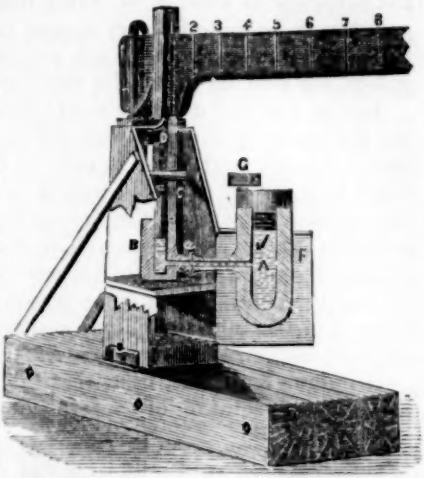
It is thought that a tabulated statement of these explosion, giving briefly the cause of each, with a general description of the boiler in every case, would be of interest, and therefore it is proposed to give this in the Annual Report for the year 1863, which will be presented to the subscribers at the ninth annual meeting now shortly to be held; and it need merely be added, therefore, in the meantime, that a review of the explosions which have occurred during 1863, clearly shows the value of periodical boiler inspection, and supports the principle advocated in the previous reports, viz:—that the ruling cause of boiler explosions is neglect, and their preventive, simply, care.

Thickness of the Earth's Crust.—Extract from a Lecture by W Fairbairn, Esq.

From the London Mechanics' Magazine, May, 1864.

It has always been a question of interest to geologists to ascertain the thickness of the earth's crust, or at what depth under the surface it becomes fluid. To determine, or rather to approximate, the solution of this question, my friend, Mr. Hopkins, of Cambridge, Dr. Joule, and myself, were requested by the British Association, to institute a series of experiments on pressure and the conductivity of the different strata with which we are acquainted. This very interesting inquiry

has extended over a series of seven to eight years, and the results obtained are of that nature which, I trust, will justify me in bringing the subject before you. The whole of the experiments have been carried on at my works in Manchester, having, at the time, a powerful apparatus, as shown in the engraving.



A is a strong cylinder of brass, with a branch communicating with the cylinder B, into which works a steel plunger *c*, which fits into the socket in the bar D. On the top of this bar the powerful lever E is applied, which, pressing on the liquid in the cylinder B, forces it through the bore of the pipe *a* (on the principle of the Bramah), and by which nearly any amount of pressure may be attained. On the top of the cap of the cylinder A, which is made air and water-tight and inserted in the bath F, a mariner's compass G, is placed, the use of which we shall presently describe.

Having adjusted the apparatus, and filled the cylinder A with the material to be experimented upon, a charcoal fire is lighted under the bath, and before applying the lever the point of the fluidity is ascertained. This was accomplished with great difficulty in the first instance, as we had no means of ascertaining, with anything like accuracy, the melting point, until Professor W. Thompson, of Glasgow, who happened to be present at one of the experiments, suggested the compass, which answered the purpose admirably. This ingenious application was effected by inserting a magnetic needle in the solid mass to be experimented upon, and, having brought the needle of the compass within its influence, it pointed directly over the centre of the cylinder, and was retained in that position so long as the material was in the solid state; but the instant the melting point was arrived at, by increasing the temperature of the bath, the needle, no longer supported by a solid substance, dropped through the fluid to the bottom of the cylinder, and, the instant that was accomplished, the needle of the compass turned round to the pole. From this arrangement it will be seen that the temperature of fluidity was carefully and accurately determined.

Leblanc's Water Gauge.—Patented by DESIRE FRANÇOIS LEBLANC.

From the London Mechanics' Magazine, September, 1863.

We feel much pleasure in laying before our readers particulars of a glass water-gauge which is extensively used in France, and deserves general introduction here. It is not only stronger and safer perhaps than any other gauge which shows the water level through a trans-

parent medium, but it possesses the great advantage of always denoting the presence or absence of water from the glass tube. The ordinary gauge may be quite full yet appear empty, or quite empty and appear full. It is impossible that such a result can take place with Leblanc's gauge, for reasons which the specification will best explain:—

Let us consider a rather thick-sided glass tube half-filled with water we shall readily note the wide difference in refraction which exists between the full portion of it and the empty part thereof; and now let there be a screen provided with a circular hole of about two-thirds of the outside diameter of this tube placed behind that portion of it which is full of water, and this circular hole will appear in the form of an ellipse, having its great or transverse axis perpendicular to the axis of the tube, whilst on the same screen being removed behind the empty portion of the tube the ellipse assumes a rather lenghtened form, having then its great or transverse axis parallel to that of the tube.

It follows from this, first, that such a screen may be used to inform us—when no solution of continuity appears in the tube—whether this is quite empty or quite full.

FIG 1.

FIG 2.

And secondly, that however darkened its sides may be, unless they are altogether opaque, it will always be possible for any one to ascertain with its aid the presence or the absence of the liquid inside.

A metallic tube pierced from end to end with circular holes opposite to each other on two or three alternate diameters and encasing the glass tube, may therefore become at once both a protection against breakages and their effects, and a means of more distinctly showing the height of the liquid inside.

Such is the nature and object of the said invention for "A Dioptical Metallic, Level Indicator or Liquid Gauge," but essentially consisting in the application of the combined dioptical properties of glass and water to the construction of a liquid gauge that should possess such requisites to clearness of indication and security of service as have never been as yet attained, it is susceptible of any form; for example, a glass tube consisting of several short ones may be advantageously substituted for that of one piece, and lenses for either of them.

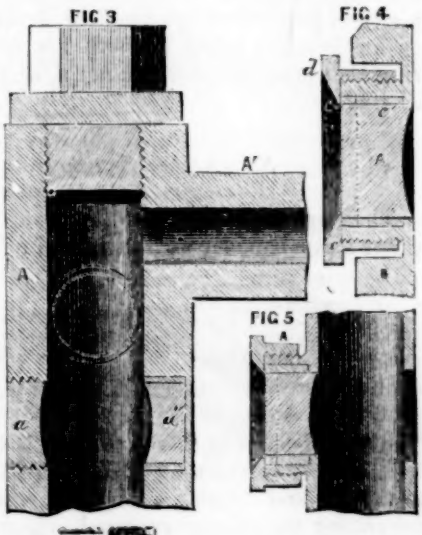
Again, a casing made of any flexible matter, as vulcanized or hardened india rubber, or simply sheets of paper or cloth, impregnated with siccative greasy substances, rolled several times and stuck on the glass tube, may also be used instead of the metallic casing.

Being given a quadrangular opaque tube, the sides of which are pierced with two circular holes opposite to each other; if you fix in each of them a plano-concave lens, whose concavity faces the axis of the tube, and whose focus lies a little behind the opposite side; the hole that faces the lens which is looked at will appear greatly reduced in diameter, the tube being empty; whilst on the same being filled with water, the index of refraction of this being pretty nearly the same as that of glass, the concavity of the lens is found to be almost annulled by the convexity of the water, and the opposite hole re-appears with its nearly full diameter. The crown glass gives the maximum of differential effect.

Figs. 1 and 2 of the foregoing drawings represent a glass tube enveloped in a metallic casing pierced with circular holes opposite to each other. The metallic casing may be simply a non-adhesive case surrounding the glass tube, but it is better to have it fixed thereon, by means of a mastic or cement, such as that of ceruse and minium, oxychloride of zinc, and such like.

A is the glass tube; B, the metallic casing; *c*, the mastic; *d*, the holes of the casing; *e*, the outline and direction of the ellipse, as seen in the empty part of the tube; e^1, the same as seen in the part occupied by the liquid.

Indicators of unlimited length and best fitted to resist expansion will be obtained by using short glass tubes joined end to end. For this purpose it is necessary that each joint should correspond with the middle of a space without holes, so long as the sealing should prevent leakage.



The mastic of ceruse and minium, by reason of its solidifying slowly in a cold place, appears to be the most suitable one to employ. The complete solidification is obtained by its being exposed for two or three hours to a temperature of from 200 degs. to 250 degs. centigrade. All mastics that solidify slowly in a cold place and quickly in a hot one may likewise be advantageously used. Colored mastics, and above all those colored with black, render more distinct the perception of the optical phenomenon.

The glass tube extends a little beyond the metallic casing, in order

to obtain a hermetical joint, in case the sealing of its extremities to its casing should not be quite water-tight, and thus to prevent acids and other agents from attacking the metal or the mastic. This disposition, however, is not indispensable, and in most cases the glass tube will be level with its casing.

Fig. 3 represents the section of the indicator, provided with lenses. A, metallic body; A', tubulatures, capable of receiving cocks; *a*, *a*', *a*'', plano-concave lenses. There are many means of fixing the lenses in the holes; they may be screwed with an oily mastic, as in *a*', or simply sealed with a mastic, specially that of cast iron in brimmed holes, as in *a*'', or only hot fixed by means of a sufficiently fusible enamel, provided that the glass made use of be of a dilatation pretty nearly the same as that of the metal employed, which should be little dilatable itself, such as cast iron, iron, steel. I do not mention platinum because of its high price.

Lenses may also be placed on thinner tubes by means of tubulatures, A, fig. 4, into which they are fixed, either with a mastic or with india rubber, by means of a screw cap. They may likewise be sealed with an enamel into very thin tubulatures which will attenuate the effects of expansion. By enamelling the cast iron, iron, and steel tubes, they will thoroughly be kept from oxidation.

Fig. 5 represents an enlarged view of a mode of fixing the lenses in the tubulatures.

A, lens; B, side of the tube; *c*, externally screwed tubulature; *c*, matter of the joint, which matter may be, according to the case, either a ring of vulcanized india-rubber, or amianth, impregnated with mastic or simply oiled hemp for both low and mean pressures; *c*'', metallic ring that compresses the joint; *d*, screwed cap, screwing on the metallic ring.

Self-Acting Spanners or Screw Wrenches.

From the London Artizan, June, 1864.

These spanners are a contrivance of Mr. L. Schwartzkopff, of Berlin, whose steam hammer we illustrated in *The Artizan* of September, 1862.

A

The main feature in these spanners is the simplicity of their construction, as they can be used for nuts of all sizes by simply applying them to the nut and turning a hand-lever, without any special mechanism for *setting* them being required. The chuck *a* is fixed on the hand-lever, and connected with the small lever *b*, which is set in motion by the movable jaw A being pressed against one side of the nut. By this means the spanners will close themselves, irrespective of the gauge of the nut, so long as the latter can be contained between the two jaws.

Refrigerating and Ice-Making Machines.

Report on the apparatus by M. Carré, having for its object the production of artificial cold.

(*Commission composed of MM. Regnault, Balard, and Pouillet, reporter.*)

From the Practical Mechanic's Journal, October, 1863.

The following valuable Report of a Commission of the Institute of France, upon M. Carré's ice-making machinery, presents many considerations more or less generally applicable to all machines of this class; and as placing some of the more fundamental points relating to these machines, as yet so imperfectly understood by mechanical engineers in this country, in a very clear light, we have deemed it fitting to supply our readers with a translation of the entire report.

M. Carré has presented to the Academy the drawings and description of an apparatus which he has invented for producing cold, and for solving the problem of artifical cold in all its generality, whether it be proposed to make ice, or simply to cool large volumes of air or liquid, or finally to cause very considerable masses of liquid to fall to the low temperatures of 15° C., (5° Fahr.,) or 20° C., (4° below zero, Fahr.,) for he is endeavoring to apply this invention to producing great degrees of cold in the sea water of some extensive salt works in the south of France.

The Commission met several times in the workshops of MM. Mignon and Rouart, where there was a model of this apparatus. They examined its construction, and further, they made it work under their eyes, as often and as long as they wished, in order to appreciate its work.

This model had dimensions large enough to render it possible to take into account all the physical and mechanical phenomena which conjoin to the result, and which insure the regularity of its working for whole days together.

The most of its operations are for the purpose not of cooling brine, but of making ice; and at its ordinary rate of working, the apparatus gives 25 kilogrammes an hour or 250 kilogrammes a day. Its refrigerating power then was 2500 units of heat (calories) an hour, since water, taken at ordinary temperatures, must lose nearly 100 units per kilogramme in its transformation into cylinders of ice, the very interiors of which are several degrees below zero.

In estimating, as we have just done, the power of the apparatus by the number of units of heat which it is capable of abstracting per hour from the body which is given to it to cool, it must not be lost sight of that there is still another element which must be taken into the account namely, the *intensity* of the cold produced. In fact, an apparatus which takes 2500 units per hour from a body in causing it to descend, for example, from 10° (50° Fahr.) to 0° (32° Fahr.,) is in no way comparable to another apparatus which takes the same 2500 units per hour but makes it descend from 20° to 30°. It is absolutely necessary then, to express in a complete manner the refrigerating power of a given apparatus, to mention at the same time the number of units which it

abstracts per hour, and the two limiting temperatures between which the cooling is accomplished.

The general principles on which the construction of the apparatus of M. Carré rests are very simple. They are put in practice in the various cryophores and refrigerators hitherto invented. They must be here referred to.

A more or less volatile liquid is contained in an hermetically sealed vessel, analogous to a steam boiler, but with this difference, that instead of receiving the heat of the fire, it gives out cold all round it. This vessel, which we shall call the refrigerator, communicates, by a large tube and stop-cock, with an empty space, which we shall suppose at the commencement to be very large. At the moment of opening the stop-cock, the vapors of the liquid rush into the void by their elastic force, other vapors form on the instant, which rush in in their turn, and the operation thus goes on as long as there is any liquid to vaporize. These vapors cannot form without taking from the walls of the refrigerator all the latent heat which is necessary for their existence and their elasticity. Thus the refrigerator becomes cooled more and more, and to establish an equilibrium temperature, it abstracts from the bodies which touch and surround it the whole quantity of heat which it has had to furnish for the evaporation. If there is formed, for example, according to the capacity of the refrigerator, 10 or 100 kilogrammes of vapor an hour, the number of units abstracted will be

5000 or	50,000,	if the	volatile liquid is	water;
2000 or	20,000,	"	"	alcohol;
900 or	9,000,	"	"	ether;

for the latent heats of these liquids are about 500, 200, and 90 units per kilogramme.

As to the degree of cold to which the refrigerator can reach by these spontaneous evaporations, it depends principally on the nature of the volatile liquid. By supplying it with water, we with difficulty get to a few degrees below zero, because solidification without wholly depriving it of the property of giving off vapors, does deprive it of the property of giving them off in great abundance. On the contrary, alcohol, ether, and other volatile bodies which remain liquid at very low temperatures, continue also capable of giving off abundant vapors, and consequently of producing very great degrees of cold.

What is thus so simple in theory becomes strangely complicated when it comes to be put in practice, when it is necessary to embody these primary ideas, in such a way as to build up a great continuously working machine, working with regularity, and governing itself almost like a steam engine. This was truly a difficult question, and M. Carré has at last given us a satisfactory solution of it.

Let us first point out the principal difficulties, or rather the points on which they bear.

1. We have supposed the refrigerator was put in communication with an empty space, indefinitely great, and that the vapor could form without ceasing, in virtue of the elastic force which is proper to it.

This hypothesis is not realizable. We must then recover this vapor as fast as it forms, and further compress, liquify, and collect it, to employ it again, because it would cost too much if it were allowed to be lost.

2. It is necessary to introduce into the refrigerator a weight of liquid equal to the weight of vapor which forms in it in a given time, and which can be pumped out of it, as we have just said. This is the rigorous condition, without which the action of the apparatus could not be regular or continuous.

3. It is necessary that all the joints and closings should be hermetically tight. The smallest quantity of air penetrating to the interior would suffice to compromise all, and the same would happen if the vapors could escape outwards.

4. In proportion as the limiting temperature at which the refrigerator ought to be lowered, the elasticity of the vapor decreases, and the volume which an equal weight occupies becomes greater and greater. Accordingly, when we want to form a given weight in a given time, for instance, 10 or 100 kilogrammes an hour, it is plain there will be a whole series of experiments to make on the forms and dimensions to be given, not only to the interior of the refrigerator, but to the supply and delivery pipes, the cocks, and in one word, of all the parts which assist in the formation or circulation of the vapor.

5. Finally, although certain complex liquids, as the solution of ammonia in water, present certain marked advantages in some respects, they are subject to all the preceding difficulties, and besides, are subject to difficulties of another nature, depending on the two vapors which they form, besides the necessity of regulating the variable proportions of their mixture.

The apparatus with which we are occupying ourselves in fact contains an ammoniacal solution as the liquid for producing the cold, and we ought consequently to expect to find in it all these difficulties at once.

Accordingly, we must attempt to give some idea of its construction, at least as far as we can without the aid of figures.

The ammoniacal solution undergoes four changes of state.

It is vaporized by a boiler.

This vapor is condensed by a liquifier. In this new state the liquid is received into a distributor, which introduces or distributes it in proper quantity into the refrigerator.

Here the liquor evaporates afresh, to produce the cold.

6. These new vapors are withdrawn through a large tube, and condensed in an absorbent reservoir, where they are exposed to the impoverished liquid drawn from the boiler itself. The poor liquid becomes rich by the absorbtion of the ammoniacal vapor, and submitted to the double effect of an exhausting and condensing pump, which exhausts it from the bottom of the absorbent reservoir, to condense it in the boiler from which it started, partly in the state of vapor, and partly in that of liquid.

Thus every thing is reduced to a complete circulation of the volatile liquid, of which the two elements, water and ammonia, are alternately

separated and re-united, either by condensation or evaporation, their mutual affinity playing here an important part, which is worthy of remark.

In order better to understand how this circulation goes on indefinitely and always with the same original quantity of liquid, we shall separate it into two parts, namely, *The passage from the boiler to the refrigerator* and *The passage from the refrigerator to the boiler.*

Passage from the boiler to the refrigerator.—In the model of the size for 2500 units per hour, the work of which we have been enabled to observe, the boiler is a vertical cylinder, of 1 m. 20 c. m. high, by 0 m. 40 c. m. in diameter. With a capacity of 1 hectolitre and a half, (=3·2 gallons nearly) it is charged with only 80 or 90 litres of a very concentrated solution of ammonia. It is maintained at a temperature which does not exceed 130°, (266° Fahr). Thus the tension of the united vapors of water and of ammonia is that of 8 atmospheres.

The upper half of the boiler is outside the furnace, and in contact with the air. It is furnished inside with a series of vessels, superimposed on one another, forming a sort of rectifying cascade, where the ammoniacal vapor frees itself in great part of the watery vapor which it contained.

This dried vapor escapes by a long tube of suitable section, which conducts it to the connector (chevet) at the entrance into the liquifier.

The liquifier is composed of four worms, plane and parallel, placed 5 centimetres from one another. The tube of each worm opens into the entrance connector, which is horizontal. It then continues in a right line, to a length of 1m.50, with the slope necessary for the flow of the liquid; there it curves round to return, always descending in the same vertical plane, makes a second ply, and again a third ply similar to the first, at the end of which it opens into the exit connector, which is parallel to that at the entrance. This system of zigzag worms is plunged in a great vessel of cold water, which is renewed sufficiently to prevent the temperature rising to 30° (86° Fahr.) by reason of the condensation of the vapor which takes place inside the tubes.

The exit connector of the liquifier receives also all the liquid which has been formed in the worms, more from the effect of the cooling than from the effect of the pressure of 8 atmospheres of the boiler, a pressure which transmits itself directly, and without any break, up to the point at which we have now arrived. Here the transformation is complete, and for the rest of the passage, and up to the refrigerator, it is liquid that circulates; but it is not the less subject to the pressure of the boiler, because it has no further obstacle to modify its free progress.

This liquid must not arrive at the refrigerator except in the most exact proportion, and with perfect regularity; hence a distributor is necessary, which shall regulate its supply.

The distributor is a cylindrical vessel, of 25 or 30 centimetres high, having a capacity of 4 or 5 litres, and carrying near the top a lateral tube for the entry of the liquid. A pipe starts from the bottom of this vessel, and is prolonged downwards, and with a common axis with the cylinder. It is 15 or 20 centimetres long, and only 2 centimetres in-

terior diameter, except at the bottom, where it is contracted for a few millimetres, and ground so as to make a sort of valve seat. There it is closed, and carries on the side, near the middle of the height of the valve seat, a small opening for the exit of the liquid.

A light and delicate float, open above and closed below, with the exception of a hole which corresponds with that of the valve seat, can move freely in the vessel, with which it corresponds in form, except that it is a little narrower, and only touches it for the height of the valve seat. The whole movement of the float is reduced to a vertical oscillation, which cannot exceed 10 or 12 millimetres, and which it performs without being able to turn on its axis.

The following is the manner in which this ingenious distributor acts. A tube establishes free communication between the exit connector and the interior of the distributor. The first liquid which comes off falls between the sides of the vessel and those of the float; the latter is soon raised, and its aperture ceases to correspond with that of the valve seat. The liquid continuing to arrive, its level overtops the edge of the float, which from this instant fills more and more. When it is half full or nearly, its weight exceeds that of the liquid which it displaces; it then descends, and at the moment it comes to a position of rest, its aperture corresponds with that of the valve seat, and the liquid escapes outwards. From that it lightens itself more and more. If the exit connector of the liquifier does not compensate the loss which it produces, it will continue to ascend, and to stop the distribution, which it is its business to make to the refrigerator. But as will be seen, this suspension can only take place when it becomes necessary, that is to say, when the reserve liquid is nearly exhausted.

To the exit aperture of the distributor is fitted a tube of small diameter, of any length or degree of curvature, which ultimately carries into the interior of the refrigerator the liquid produced by the cold, and which thus terminates the first part of the circuit. This tube, before entering the refrigerator, is furnished with a stop-cock, which is the first that is met with from the time of leaving the boiler, or the starting point of the circuit. To show how necessary this arresting point is, it will suffice to remark, that the tension of the vapor in the refrigerator should be about one atmosphere, or perhaps a little more, as we shall see immediately, while it is eight atmospheres in the boiler.

With this excess of seven atmospheres, then, the vapor in the boiler would rush violently into the refrigerator, if this first stop-cock were not interposed. It is, then, indispensable. It is this which controls the effect of this excess of pressure, which stops it off at will, and which reduces it to what it ought to be, in order that the liquid may be injected into the refrigerator at a suitable rate.

It would be superfluous to describe the refrigerator here, because its form and dimensions depend on the effects which it is wished to produce. They will be very different, whether it be intended to make ice, or to cool masses of liquid, which are changed with more or less quickness. We shall confine ourselves to saying that the form of the refrigerator is far from arbitrary, and that in every case there are two

essential conditions to which it remains subject, namely, to expose large evaporating surfaces always wetted with liquid, and at the same time, to give a very free circulation to the vapor, and also to collect in a small space the residues of the evaporation, which become more and more hydrated, and from which it is necessary to free the refrigerator from time to time, by certain and easy means.

(To be Continued.)

On the Supposed Nature of Air prior to the Discovery of Oxygen.

By GEORGE F. RODWELL, F.C.S.

From the London Chemical News, No. 202.

(Continued from page 30.)

2. *To the Death of Galileo.*—Science made but little progress during the middle ages, but that such was the case is not to be wondered at, when we consider the constitution of society at the time.

The chief elements in that society were the feudal barons and the priests. The barons were perpetually at war with their neighbors, war was their profession, and study of any kind was distasteful to them; their minds were never required to think deeply, and soon lost the power of doing so for want of practice. The priests were often men of learning, but they devoted their energies solely to theology, for their church considered the scientific dogmas of Aristotle sufficient for all purposes, and forbade speculations in reference to the causes of natural phenomena. It was the church therefore which raised obstructions to prevent the further progress of science, it was the church that did not allow men to think for themselves, and which undoubtedly by so doing retarded the civilization of Europe for centuries. We have not yet recovered from the effects of that edict.

There were, no doubt, great and original scientific thinkers during the middle ages, but their thoughts too often perished with them, for they did not dare to oppose Aristotle openly, and but little thought must have shown them the fallacy of his reasoning.

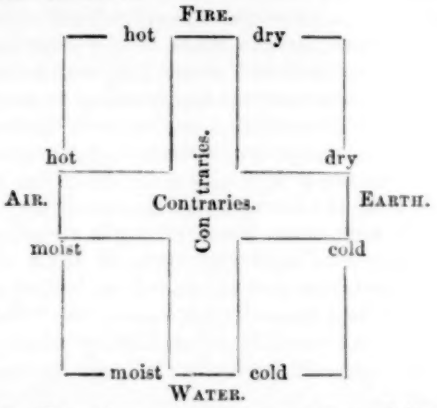
The alchemists in their search for the philosopher's stone collected together a great mass of matter, out of which has been extracted a few facts of practical utility, for labor applied to the elucidation of the most visionary and impossible theory is never entirely lost; purged of its dross, a little globule of pure metal always remains. I believe if a man were to try to harness a comet to a railway train, although ninety-nine parts of his labor would be lost, the hundredth would be of profit to the world.

In the writings of the alchemists we usually find a chapter devoted to the four elements, and the conversion of one into the other, according to Aristotle's theory, is generally represented by a diagram. The following is taken from a work on Alchemy* published in 1546:—

Fire is represented by an angel, air by a bird, water by a dragon, and earth by a bull.

* "Pretiosa Margarita Novella de Thesauro," &c., 1546.

The manifestation of a taste for scientific studies was very apparent in Italy in the middle of the sixteenth century, and several scientific societies were then founded.



Giovanni Baptista Porta* was one of the greatest cultivators of science of this period; he was the author of numerous works on a variety of subjects; among them I may mention several works on physiognomy, several comedies, and treatises on "The Art of Remembering," "On Distillation," "On Refraction," "On Natural Magic," and "On the Transmutations of the Air;" the two last alone claim our attention.

The treatise on "Natural Magic" is Porta's best known work. In the preface to the edition which I have consulted, Porta informs his readers that he first published the work when he was scarcely fifteen years old, and that it was well received and translated into Italian, French, Spanish, and Arabic; he now sends it forth again to the world thirty-five years later. The contents were taken both from the writings of the ancients and from those of his contemporaries; libraries in France Italy, and Spain were ransacked; Porta's own personal observations in various countries were added, and he was assisted by "nobles, potentates, great and learned men;" moreover, money was willingly contributed to pay for the expenses of trying experiments.

Let us leave the preface, and look at the result of all this labor.

The Treatise is divided into 20 Books.

Book 4 treats "Of increasing Household Stuff."

Book 7 gives a good account of the principal properties of the loadstone, and is the most philosophical treatise in the whole work.

Book 8 "Of Physical Experiments," is a very curious production. We find among the experiments—"To make a man out of his senses for a day," "To fasten the teeth," and an account of "A preservative against envy."

Book 9 treats "Of Beautifying Women."

Book 13 "Of Tempering Steel."

Book 14 "Of Cookery."

Book 20 "Of the Chaos."

* Born, 1545. Died, 1615

This work gives us some idea of the scientific literature of the period. Here was a man of undoubted literary tastes, with every possible facility given him for the compilation of his book, with thirty-five years to collect and arrange his subjects, and to build them up on a foundation already laid, with the assistance of the most learned men of the time; in a word, furnished with everything which could make his work the most scientific which had yet appeared, and when he gives it to the world he is found to have written on the most diverse and eminently unscientific subjects, which are, moreover, badly arranged and badly handled. Still this work was a step in the right direction; Porta's predecessors had for the most part written on sorcery and necromancy, and had dealt in such arts; Porta strongly repudiates the assertion that he was a sorcerer, and endeavors to show that the magic of nature is as wonderful as that of wizards and witches; and as a work written for such a purpose, we must regard the "Natural Magic," in spite of its faults, as one of the channels by which the human mind escaped from the stagnant lake of middle age ignorance and superstition.

Porta's treatise on the "Transmutations of the Air," contains a table which begins with air and ends with forty-three entities, into which he believes it can be transmuted; the following are among them:-

Air vehemently agitated by the orb of the moon, becomes fire. Rarefied by the heat of the sun—wind. Moderately condensed—Clouds. Congealed—snow.

Rarefied by heat beneath the earth, air produces earthquakes, and by other causes it becomes dew, meteors, thunder, lightning, whirlwinds, &c., &c. This work contains chapters on rain, snow, hail, clouds, and on other subjects connected with meteorology.

Our own countryman Francis Bacon,* did more to engraft in the human mind a love for science than any of his predecessors. He was the founder of a new epoch in the history of science, the advocator of a new method of addressing questions to Nature, the very father of all our experimental sciences. Not only did he give us the true method of conducting scientific inquiry, but he also put many of his own suggestions to the test by actual experiment; a few of his experiments relate to the air.

Bacon believed that air was produced from water, for he tells us that a drop of water turned into air occupies a hundred times its former bulk; and he considers it very probable that air may be condensed to water by subjecting it for some months to a freezing temperature, because we see that solid bodies are permanently affected by being kept in a state of strain; a stick if kept bent for a considerable time does not regain its former shape; may it not happen that the air will be permanently affected by violence?

In the forty-fifth aphorism of Book II. of the "Novum Organum," Bacon mentions an experiment which he made to discover to what extent air could be rarefied. He took a glass egg, and withdrew by suction as much air as was possible by the means; the orifice was then

* Born, 1561. Died, 1626.

closed by the finger, and the egg immersed in water, which, on opening the orifice, of course rose to a certain height in the egg, the reason of which he explains thus :*—"The air being constrained by the effort made in suction, and dilated beyond its natural state, and therefore striving to recover and contract itself (so that if the egg had not been immersed in water it would have drawn in the air with a hissing sound) now drew in a sufficient quantity of water to recover its former dimensions." It is difficult from this explanation to understand in what light Bacon viewed the rushing in of water; he seems to have considered that suction acted on the air in the egg as force acts in bending a stick; and the contraction of the air an effect similar to that of a bent stick regaining its shape, and it was natural to compare one elastic body with another. We must bear in mind that he knew nothing of the pressure of the air, for he considered it imponderable.

It would be a great advantage, he writes, if bodies could be endued with fixed properties "by violence," and he recommends it to be tried, if, when a glass egg is exhausted of air by suction, the orifice closed, and the whole put aside for some days, the air rushes in with a hissing sound on opening the aperture, and if as much water as before enters the egg.

If onions are hung up in the air they are found after a time to have sprouted. Bacon recommends that they should be weighed before and after sprouting; if they have gained weight it will be a proof that "air may be made to condense, and also that air can nourish."

There can be no doubt, writes Bacon in the "Sylva Sylvarum,"† that the airs differ in different localities, because it is well known that "confitures and pies" become mouldy, and meat rots in some airs much sooner than in others. The tests which he gives for the purity of air are curious. Air may be considered wholesome if wool hung up in it does not increase much in weight, if meat does not rot quickly, and if the weather-glass undergoes no sudden change. The weather-glass here mentioned had been but recently invented; it was the instrument which we now call the air thermometer, and therefore showed changes of temperature as well as changes of the air's pressure. There has been much dispute as to who invented this thermometer; some attribute it to Santorio, of Padua, others to Cornelius Drebbel, but the generally received opinion seems to be that it was invented by Galileo in 1597.

Galileo‡ was the contemporary of Bacon, and we occasionally find him quoted in the "Novum Organum."

He entered the field of science at a time when, from neglect, it was overgrown with all manner of rank and stunted forms, and required a strong hand to chop and cut right and left to remove the weeds, and to plant new seed. Galileo attacked the dogmas of Aristotle, and proved their fallacy, but with the very proofs before their eyes, men refused

* According to J. Devey's translation.

† Published after the author's death by W. Rawley. Tenth Edition, 1676, consulted.

‡ Born, 1564. Died, 1642.

to admit the new doctrines, and it was not till long after the publication of the celebrated "Mathematical Dialogues," that Aristotle's physical theories received their death-blow.

In the "Mathematical Dialogues"* several experiments relative to the air are mentioned.

Nothing, he says in the first Dialogue, shows us Nature's abhorrence of a vacuum better than the experiment in which two plates of metal or marble, ground with very smooth surfaces, when pressed together, are found to adhere with such force that we can lift one by the other. In order to show "the power of a vacuum," Galileo took a hollow cylinder of metal, smoothly turned inside, and closed at one end; into this he fitted accurately a short solid cylinder of wood, to which a hook for supporting weights was attached. The metal cylinder was filled with water, and the wood pressed down upon its surface; on now inverting the vessel he found that a considerable weight might be attached to the hook without separating the wood from the water, the surface of the wood was connected with the water according to Galileo, by no other cause "but the repugnance to a vacuum," and if we know the weight of the wood, the hook, and the added weights, we obtain the "quantity of the force of the vacuum."

It is curious that a philosopher with these facts before his eyes could have failed to discover the pressure of the air; but we arrive now at the observation which ultimately led to the discovery of that great fact.

On one occasion Galileo observed a cistern, to draw water out of which was a pump; "this pump," he writes, "had its sucker and valve on high, so that the water was raised by attraction and not by impulse as it is in those pumps which work below. This drew the water in great plenty whilst the water was at a certain determinate height in the cistern, below which, if the water subsided, the pump was useless. The first time I observed this I thought the engine had been broke. I sent for a workman to mend it, who told me that this defect was owing to nothing but to the waters being too low, and thence not suffering itself to be raised to such a height; and, he added, that neither pump nor any other machine which raises water by attraction could ever raise the water a hair's breadth more than eighteen cubits; and, let the pump be big or little, that this is the utmost height. Now, although I knew that a rope, a wooden staff, and an iron rod might be so lengthened that, lifting them upright, they would break by their own weight, yet I was so silly as never till now to consider that the same thing would much more easily happen in a string or thread of water; and what else is that which is attracted within the pump but a cylinder of water, which, being more and more prolonged, at length arrives at that term beyond which, if it be elevated, it, like a rope, is broke by its own excessive weight?" Such was Galileo's explanation of the phenomenon which led to Torricelli's discovery of the pressure of the air.

Galileo believed that the particles of matter are held together by having a vacuum between them, penetrable by fire but not by air.

* First published in 1632; done into English by T. Salisbury in 1661, and by Thomas Weston in 1730. Weston's translation has been consulted.

Galileo found that air retarded the velocity of a projectile, and he proved the "difference of velocities in movables of different gravities to be more and more as the mediums are more and more resisting;" and hence, he continues, "I have thought that if the resistance of the mediums be wholly taken away, all matter would descend with equal velocity."

Galileo was the first to prove satisfactorily that the air possessed weight. In order to show this he took a glass bottle with a narrow neck, fitted it with a leather cover and valve, and by means of a syringe compressed a quantity of air within it; the bottle and compressed air were now carefully weighed, the weight being adjusted by fine sand; he then opened the valve, and thus let out the compressed air. On re-weighing, the bottle was found to be lighter, and a quantity of sand had to be removed from the other scale. There can be no doubt, he says, that the sand removed represents the weight of air which was violently forced into the bottle.

Galileo made use of two methods to determine the specific gravity of the air. By the first compressed air is caused to form water from a vessel; by the second a vessel is taken, and a quantity of water forced into it without allowing any air to escape. The air thus suffers compression; the whole is now weighed, the valve opened, and the compressed air allowed to escape. The air which escapes is obviously equal in bulk to the water in the bottle, and, by again weighing, the weight of air which has escaped is found; the water is also weighed, and thus we arrive at the relative weights of the two.

(To be Continued.)

French Cement.

From the London Chemical News, No. 231.

This cement composed of lime and india-rubber, is very valuable for mounting large microscopical preparations. The principle advantages are—that it never becomes perfectly hard, and thus permits considerable alteration to take place in the fluid contained in the cell without the entrance of air, and it adheres very intimately to glass, even if it be perfectly smooth and unground. If a glass cover is to be affixed to a large cell containing fluid, a small piece of the cement is taken between the finger and thumb and carefully rolled round until it can be drawn out into a thread about the eighth or tenth of an inch in thickness; this is applied to the top of the cell, before introducing any fluid and slightly pressed down with the finger previously moistened. It adheres intimately. The preservative fluid with the preparation are now introduced, and the cell filled with fluid, which indeed is allowed to rise up slightly above the walls. The glass cover, rather smaller than the external dimensions of the cell, and slightly roughened at the edges, is to be gently breathed upon, and then one edge is applied to the cement, so that it may be allowed to fall gradually upon the surface of the fluid until it completely covers the cell and a certain quantity of the superfluous liquid is pressed out. By the aid of any pointed

instrument a very little cement is removed from one part, so that more fluid may escape as the cover is pressed down gently into the cement. The pressure must be removed very gradually, or air will enter through the hole. A bubble of air entering in this manner may often be expelled again by pressure, or it may be driven out by forcing in more fluid through a very fine syringe at another part of the cell, but it is far better to prevent the entrance of air in the first instance. The edge of the glass cover being thoroughly embedded in the cement, the small hole is to be carefully plugged up by a small piece of cement, and the cell allowed to stand perfectly still for a short time, when it may be very gently wiped with a soft cloth. The edges of the cement may be smoothed by the application of a warm iron wire, and any superabundance removed with a sharp knife. A little Brunswick black or other liquid cement may be applied to the edges for the purpose of giving the whole a neater appearance. The cement is made as follows:—A certain quantity of india-rubber scraps is carefully melted over a clear fire in a covered iron pot. When the mass is quite fluid, finely powdered lime, having been slacked by exposure to the air, is to be added by small quantities at a time, the mixture being well stirred. When moderately thick, it is removed from the fire and well beaten in a mortar, and moulded in the hands until of the consistence of putty. It may be colored by the addition of vermilion or other coloring matter. This cement answers well for fixing on the glass tops of large preparation jars, but if moderately strong spirit be used a little air must be permitted to remain in the jar.—*From "How to work with the Microscope," by Lionel S. Beale.*

On the Pressure of Steam at High Temperatures.

By R. A. Peacock, C.E.

From the London Artizan, Jan. 1864.

Some years ago the writer had occasion to attempt to calculate the probable pressure of steam at the highest known temperatures and found, amongst other things, that between the pressure of 25 lbs. per square inch and 300 lbs. to the square inch, the latter being the highest pressure to which reliable experiments had been carried, the law of increase was approximately as follows:—

1. The *temperature* of high pressure steam of, say, 25 lbs. to the square inch and upwards, increases as the $4\frac{1}{2}$ *root* of the pressure.

2. Conversely the *pressure* of steam of, say, 25 lbs. to the square inch and upwards, increases as the $4\frac{1}{2}$ *power* of the temperature.

3. At lower pressures than about 25 lbs. per square inch, a different law prevails.

It may, perhaps, be of interest to some to see the calculations by this formula placed side by side with the results of some of the best known experiments and formulæ. And on a future occasion the working formula, which is very simple and short, will also be given.

The following table gives a copy of the "more trustworthy of Arago and Dulong's experiments after all necessary corrections," copied from "A Treatise on Heat" by the Rev. R. V. Dixon, A.M., Dublin, 1849,

p. 173, and copied by him from "Ann. de Chim. et de Phys." tome xliii, p. 108, reduced, however, from Cent. degrees to Fahr., and from elastic force in atmosphere of 0·76″ to pressure in pounds per square inch by the present writer, an atmosphere being taken at 14·7 lbs. It will be seen that the formula does not agree with MM. Arago and Dulong's experiments, but that it does agree nearly with other well-known experiments specified afterwards.

The American Commissioners' experiments, which are not now given, differ as much from the formula as MM. Arago and Dulong's, but in a different way.

MM. Arago and Dulong's Experiments.

Pressure.	Temperature Fahrenheit Observed.	Temperature Fahrenheit Calculated.	Differences.
lbs. per sq. in.	Deg.	Deg.	
31·458	254·66	252 83	+1·83
42·196	271·94	269·88	2·06
67·23	301·46	299·31	2·15
95·516	326·12	323·61	2·51
108·4198	335·30	332·84	2·46
170·99	371·30	368·31	2·99
252·619	404·24	401·68	2·56
254·089	405·32	402·20	3·12
272·008	410·90	408·33	2·57
316·858	425·12	422·42	2·70
351·8298	435·47	432·36	3·11

The following are the experiments of Dr. Wm. Fairbairn, F.R.S., given at p. 313, of "Useful Information for Engineers," second series. The formula, it will be seen, never differs as much as half a degree Fahr. from experiment; and the calculations being sometimes less and sometimes greater than the corresponding experiments, indicate that the experiments are averaged by the formula, and some of the differences are quite insignificant.

Pressure.	Temperature Fahrenheit Observed.	Temperature Fahrenheit Calculated.	Differences.
lbs. per sq. in.	Deg.	Deg.	
26·5	242·90	243·37	—·47
27·4	244·82	245·19	—·37
27·6	245·22	245·59	—·37
33·1	255·50	255·71	—·21
37·8	263·14	263·36	—·22
40·3	267·21	267·14	+·07
41·7	269·20	269·17	+·03
45·7	274·76	274·70	+·06
49·4	279·42	279·50	—·08
51·7	282·58	282·34	+·24
55·9	287·25	287·28	—·03
56·7	288·25	288·20	+·05
60·6	292·53	292·48	+·05

The following are M. Regnault's experiments from 23 lbs. to the square inch up to 300 lbs. to the square inch, from which the formula never differs as much as three-quarters of a degree Fahr., and at 300 lbs. pressure it very nearly coincides with experiment.

Regnault's experiments, as now given, are copied from Dr. W. Fairbairn's "Mills and Millwork," vol. i, p. 202, obtained from the tables of M. Regnault, by interpolation and reduction to English measures.

Pressure.	Temperature Fahrenheit Observed.	Temperature Fahrenheit Calculated.	Differences.	Pressure.	Temperature Fahrenheit Observed.	Temperature Fahrenheit Calculated.	Differences.
lbs. per sq. in	Deg.	Deg.		lbs. per sq. in.	Deg.	Deg.	
23	235·43	235·83	—·40	59	291·42	290·75	+·67
24	237.75	238·07	—·32	60	292·51	291·84	+·67
25	240·	240·24	—·24	65	297·77	297·08	+·69
26	242·16	242 34	—·18	70	302·71	302·01	+·70
27	244·16	244·39	—·13	75	307·38	306·67	+·71
28	246·32	246·37	—·05	80	311·83	311·10	+·73
29	248·30	248·30	equal	85	316·	315·32	+·68
30	250·23	250·17	+·06	90	320·03	319·35	+·68
31	252·09	252·01	+·08	95	323·87	323·22	+·65
32	253·94	253·81	+.13	100	327·56	326·92	+·64
33	255·70	255·53	+·17	105	331·10	330·49	+·61
34	257·47	257·23	+·24	110	334·51	333·92	+·59
35	259·15	258·90	+·25	115	337·84	337·24	+·60
36	260·83	260·52	+·31	120	340 99	340·44	+·55
37	262·44	262·11	+·33	125	344·06	343·54	+ 52
38	264·04	263·67	+·37	130	347·05	346·55	+·50
39	265·58	265·20	+·38	135	349·93	349·47	+·46
40	267·12	266·70	+·42	140	352·76	352·30	+·46
41	268·60	268·16	+·44	145	355·6	355·06	+·54
42	270·07	269·60	+·47	150	358·3	357·75	+·55
43	271·50	271·02	+·48	160	363·4	362·91	+·49
44	272·91	272·40	+·51	170	368·2	367·84	+·36
45	274·30	273·77	+·53	180	372·9	372·55	+·36
46	275·65	275·11	+·54	190	377·5	377·04	+·46
47	276·99	276·42	+·57	200	381·8	381·37	+·43
48	278·30	277·72	+·58	210	386·	385·52	+·48
49	279·59	279·	+·59	220	389·9	389·52	+·38
50	280·85	280·25	+·60	230	393·8	393·39	+·41
51	282·09	281·49	+·60	240	397·5	397·13	+·37
52	283·32	282 70	+·62	250	401·1	400·75	+·35
53	284·53	283·90	+·63	260	404·5	404·26	+·24
54	285·73	285·08	+·65	270	407·9	407·07	+·23
55	286·90	286·25	+·65	280	411·2	410·97	+·23
56	288·05	287·40	+·65	290	414·4	414·19	+·21
57	289 19	288·53	+·66	300	417·5	417·32	+·18
58	290·31	289·65	+·66				

It will be observed that the discrepancies between the formula and M. Regnault's table are not confirmed by Dr. Fairbairn's experiments. For example, with the pressure 56 lbs. in Regnault's table, we have a difference between that table and the formula of +·65, whereas between Fairbairn's table for the pressure 56·7 lbs. and the formula, we

have a difference of only +·05, and the like is observable with respect to Fairbairn's pressure of 60·6 lbs., and Regnault's pressure of 60 lbs. so that it is probable the apparent discrepancy which gradually increases up to +·73 and then diminishes again between the formula and Regnault's table, does not really exist.

The following table is given in *The Artizan* for October, 1863, p. 219, the third and fourth columns being calculated by the present writer as before.

Pressure.	Mr. Birckel's Temperature.	Temperature Calculated.	Differences.
lbs. per sq. in.	Deg.	Deg.	
24·54	239	239·255	—·255
28·83	248	247·976	+·024
33·71	257	256·745	—·205
39·25	266	265·574	+·426
45·49	275	274·426	+·574
52·52	284	283·33	+·67
60·40	293	292·27	+·73
69·21	302	301·25	+·75
79·03	311	310·264	+·736
89·86	320	319·24	+·76
101·90	329	328·285	+·715
115·10	338	337·3	+·7
129·80	347	346·43	+·57
145·80	356	355·5	+·5
163·30	365	364·564	+·436
182·40	374	373·63	+·37
203·3	383	382·75	+·25
225·9	392	391·82	+·18

This table, by Mr. J. J. Birckel, agrees very nearly with M. Regnault's.

In the November number of *The Artizan*, Mr. Macquorn Rankine, F.R.S., publishes a paper "On the Expansive Energy of Heated Water," in which he gives a table, which we quote from; and which, in its first part, consists of some of the identical figures just given under the name of Mr. J. J. Birckel.

Initial absolute Pressure.	Initial Temperature Fahrenheit.	Initial Temperature Fahrenheit Calculated.	Differences.
lbs. per sq. in.	Deg.	Deg.	
28·83	248	247·976	+·024
52·52	284	283·33	+·67
89·86	320	319·24	+·76
145·8	356	355·5	+·5
225·9	392	391·82	+·18
336·3	428	428·05	—·05
	about 2360	729,632 lbs. or 325·7 tons.	

The foregoing is Mr. Macquorn Rankine's table—the last two columns being added in the usual manner by the present writer.

(To be Continued.)

Artificial Light and Lighting Materials. By B. H. PAUL, Esq.

From the Journal of the Society of Arts, No. 593.

The use of artificial light being one of those appliances which belong to an advanced state of civilization, it has naturally undergone that kind of progressive development which is characteristic of such arts. There has been, within the last 100 years, not only an immense improvement in the methods of obtaining artificial light, but also an equally important change in the sources whence lighting materials have been derived.

Towards the end of the last century the only materials used for lighting were animal fats, such as tallow and fish oils, the former being used as candles, the latter burnt in lamps. Improvements were, from time to time, made in the preparation of these materials, by the introduction of new substances, and also in methods of using them; but those improvements, though highly advantageous, sink into insignificance when compared with the introduction of coal gas as a lighting material. In towns, the use of gas soon prevailed over the use of other lighting materials, but in the country, and especially in remote places, candles and lamps still continue to be largely used. The advantages of gas are indeed of such a nature as to require a thickly populated town or district in order to be fully realized.

One of the materials formerly used for lighting, to a small extent and in a few localities, was petroleum; and some years before the introduction of gas, attempts were made to prepare artificial petroleum from various bituminous minerals, in order to use it as a source of light. These attempts were progressing fairly, when the discovery that the inflammable gas obtainable from coal by heat, could be used for lighting purposes, was made known by Murdoch, and this new method of lighting became, for a time, almost the sole object of attention in regard to artificial light. The manufacture of artificial petroleum was, however, continued to some extent, in a few places, where circumstances were unfavorable for the introduction of gas-lighting. About the year 1830 works were established in France for this purpose, and have continued in operation to this day. Somewhat later, other works were started in other parts of the continent as well as in this country; about the year 1853, the earth oil of Rangoon was examined by Mr. Warren De La Rue and Hugo Müller, with a view to its application for lighting purposes, and about the year 1860, the extraordinarily abundant supplies of petroleum in America became the object of special interest as materials for lighting.

Chemical Nature of Hydro-carbon Oils used for Lighting.—It is mainly to the products of natural and artificial materials, known under the various names of photogen, paraffin oil, &c., that I now beg to re-

quest your attention. Whether these materials be of natural or artificial origin, they all appear to agree in possessing the same general character, and in being of the same chemical nature. The chemical history of these materials is still very incomplete; but according to the results of the best chemical investigations, they appear to consist, for the most part, of mixtures of a considerable number of hydro-carbons, which differ from each other by small degrees in physical character and chemical compositions, but at the same time present a general analogy among each other, and with the gaseous hydro-carbon known as marsh gas. This substance constitutes the initial and typical member of the series, while the other extremity of the series, are solid hydro-carbons, resembling wax or spermaceti in appearance, constituting a number of substances known to chemists under the collective name of paraffin. Between these two extremes there are a great number of other substances, solid, liquid and gaseous, some of which have been separated and examined; others have not yet been obtained in an isolated state.

In addition to the hydro-carbons above enumerated, some others, belonging to other chemical series, have been detected, in smaller and varying proportions, both in the various kinds of natural petroleum, and in the tarry oils obtained by distilling bituminous minerals, peat, wood, &c., at a temperature not exceeding dull redness. These latter also contain a considerable proportion of oxygenated oils, which partake in general of the nature of creosote. In the natural petroleum the amount of these oxygenated oils is very much smaller, and it is only in some few instances that their presence has been recognised with any degree of certainty. Both in the natural petroleum and in the oils obtained by destructive distillation, there is always more or less of a kind of pitchy substance in solution, which gives to these liquids their characteristic black and tarry appearance, and there are likewise small quantities of strong smelling substances, to which the peculiar and generally offensive smell of these materials is due.

Origin of Natural Hydro-Carbon Materials.—The very intimate chemical relation between native petroleum and the oily products of dry distillation, is highly interesting from the probability which it appears to suggest, that these materials, whether natural or artificial, have a common, or at least, a closely analogous origin. The close resemblance between petroleum and the oily products of dry distillation has long been recognised, and the actual identity of a great many of their several constituents which has been only recently ascertained, is but a more detailed elucidation of the obvious analogy between these materials. Guided by this analogy, many naturalists have come to the conclusion that petroleum, in the various states in which it is met with, is the product of a kind of natural distillation that has taken place, or may be still going on, in the interior of the earth.

Whether or not this speculation be strictly correct, it undoubtedly presents a high degree of probability. All organic substances which are not themselves volatile, such as wood, flesh, and other vegetable and animal substances, yield when subjected to the influence of heat below dull redness, tarry oils, which have in all cases the same gene-

ral character and similarity to petroleum; differing only according to the specific differences in the materials from which they may have been obtained.

The various kind of bituminous minerals met with chiefly in the coal measures, and to some extent in other geological situations, also yield, under similar conditions, oily products of the same general character. This fact at once becomes intelligible when it is remembered that the bituminous portion of the minerals from which such products are obtainable, has originated from the same kind of vegetable and animal substances that are now met with in nature. Hence, whether it be supposed that natural petroleum has been produced by an actual distillation of bituminous minerals, or by some other process, different, and slower in its operation, though effecting the same result as distillation, it is not the less evident that the ultimate sources of the minerals which serve for the manufacture of those lighting materials to which I am now referring, are in all cases the same—namely, organic substances, and those, in all probability, for the most part of vegetable origin.

There is, however, an important difference between these bituminous minerials and unaltered organic substances, in regard to their fitness for yielding by distillation such products as are suitable for the manufacture of hydro-carbon lighting materials. This is the difference that exists between the composition of unaltered organic substances and the composition of those remains of organic substances belonging to former epochs, which constitute the bituminous portion of the minerals to whose formation they have partly contributed.

Taking, for the sake of illustration, dry wood as representing the composition of the organic substances, which are the primary sources of bituminous minerals, and comparing it with the average composition of the bituminous portion of coal, it appears that for a given per centage of hydrogen the relative amounts of carbon and oxygen are widely different.

	Wood.	Coal.
Carbon,	50·00	88·15
Hydrogen,	5·55	5·26
Oxygen,	44·45	6·59
	100·00	100·00

This difference arises from the alteration the organic substances have undergone during their passage into that bituminous portion of minerals by which they are now represented. This alteration consists in a progressive elimination of the oxygen, either as water or carbonic acid or both, and the consequent concentration of carbon, as may be seen from the following representation of this change:—

	Composition of wood.		Composition of water.		Residue.		Composition of carbonic acid.		Residue.
Carbon,	50·00				50·00	—	6	=	44·00
Hydrogen,	5·55	—	1	=	4·55				4·55
Oxygen,	44·45	—	8	=	36·45	—	16	=	20·45
	100·00	—	9	=	91·00	—	22	=	69·00

This change is analogous to that which takes place in decaying organic substances, and is, in fact, a slow carbonization. Its effect is of considerable importance as regards the artificial production of hydro-carbons for lighting purposes, inasmuch as the value of any material for this purpose increases with the increase in the amount of hydrogen, and the decrease in the amount of oxygen they contain. It is on account of the large amount of oxygen in wood and peat, as compared with coal, that those materials are less suitable for the manufacture either of illuminating gas or hydro-carbon oils. In the former case the gas produced from them contains such a large amount of carbonic acid, that its illuminating power is very slight, and in the latter case, the oils produced by distillation, are contaminated with oxygenated oils and resinous substances, which render their purification more troublesome than that of the oils obtainable from bituminous minerals, in which the amount of oxygen remaining is so much less.

But there is another feature in this alteration of the organic substances from which the bituminous portion of the minerals in question has originated. By referring to the above comparison between the composition of wood and that of coal, it will be seen that not only has the oxygen been, to a great extent, eliminated, but there has been an alteration in the ratio existing between the hydrogen and carbon. Instead of being, as in the case of wood, in the ratio of 1 hydrogen to 9 carbon, it is in coal in the ratio of 1 hydrogen to 17 carbon. This circumstance is indicative of the separation, during the change that has taken place in the conversion of decaying organic remains into the constituent bituminous minerals, of a compound of carbon and hydrogen, such, for instance, as marsh gas.

This gas, consisting of carbon and hydrogen in the ratio of 3 to 1, is always produced in putrefaction, and by the action of heat upon organic substances. It also occurs naturally in connexion with the coal measures, constituting, together with small quantities of olefiant gas and nitrogen, the main portion of the fire-damp of coal mines. Carbonic acid gas is also of frequent occurrence in the coal mines. These facts, together with the close chemical relationship between marsh gas and the various constituents of petroleum, all agree in adding to the probability of a very close connexion between bituminous materials and the various kinds of petroleum; a connexion of such a nature, in all probability, that they should be regarded as a simultaneous origin, in the same way that the carburetted hydrogen or marsh gas of coal mines, has most likely originated, not from the decomposition of coal itself, but conjointly with coal, by the progressive alteration of the organic remains whence it and other bituminous minerals have been formed.

Exhalations of marsh gas almost always accompany the discharge of petroleum from natural springs, and in most of the American petroleum it is possible to recognise the presence of small portions of the other substances, ranging next to marsh gas in the above series, and gaseous under ordinary conditions of temperature and pressure.

		Carbon.	Hydrogen.	Sp. Gr.	Carbon in one gallon.	Boiling point, Fahr.	Vapor density.	Carbon in equal volumes of vapor.	Volume of air requisite for burning equal volumes of vapor.
					lbs.				
1 Methyl-hydride	C_2 H_4 (marsh gas).	75·00	25·00	—	—	gas	0·554	—	10
2 Ethyl-hydride	C_4 H_6 . .	80·00	20·00	—	—	"	1·04	—	—
3 Propyl-hydride	C_6 H_8 . .	81·81	18·19	—	—	"	1·52	—	—
4 Butyl-hydride	C_8 H_{10} . .	82·76	17·24	—	—	"	2·01	—	—
5 Amyl-hydride	C_{10} H_{12} . .	83·33	16·67	0·628	5·233	86	2·49	2·5	—
6 Hexyl-hydride	C_{12} H_{14} . .	83·72	16·28	0·669	—	158	2·97	3·0	—
7 Heptyl-hydride	C_{14} H_{16} . .	84·00	16·00	0·699	5·9	198	3·46	3·5	70
8 Octyl-hydride	C_{16} H_{18} . .	84·21	15·79	0·726	—	243	3·94	4·0	—
9 Nonyl-hydride	C_{18} H_{20} . .	—	—	0·747	—	278	—	4·5	—
10 "	C_{20} H_{22} . .	84·50	15·50	0·757	—	321	—	5·0	—
11 "	C_{22} H_{24} . .	84·61	15·39	0·766	—	359	—	5·5	—
12 "	C_{24} H_{26} . .	—	—	0·776	—	408	—	6·0	—
13 "	C_{26} H_{28} . .	84·78	15·22	0·792	—	423	—	6·5	—
14 "	C_{28} H_{30} . .	84·84	15·16	0·800	6·787	460	6·85	7·0	—
15 "	C_{30} H_{32} . .	—	—	—	—	496	—	7·5	—
16 Cetyl-hydride	C_{32} H_{34} . .	—	—	—	—	527	7·825	8·0	—
17 "	C_{34} H_{36} . .	85·00	15·00	0·825	7·022	. . .	8·37	8·5	—
18 "	C_{36} H_{38} . .	—	—	—	—	. . .	—	9·0	—
19 "	C_{38} H_{40} . .	—	—	—	—	. . .	—	9·5	—
20 "	C_{40} H_{42} . .	85·10	14·90	—	—	. . .	—	10·0	—
21 "	C_{42} H_{44} . .	—	—	—	—	. . .	—	10·5	—
						Melting point.			
25 Candle paraffin?	C_{50} H_{52} . .	85·23	14·77	0·870	7·41	130	12·19	12·5	—
27 Ceryl-hydride	C_{54} H_{56} . .	—	—	—	—	135	—	13·5	—
30 Myricyl-hydride	C_{60} H_{62} . .	85·31	14·69	0·890	—	143	14·61	15·0	—
Olefiant gas	C_4 H_4 . .	85·71	14·29	—	—	gas.	0·97	1·0	15

Characters of Hydro-Carbon Lamp Oils.—In the absence of any important difference between those constituents of natural petroleum and of the oils obtained by distilling bituminous minerals, which are capable of being employed as lighting materials, it will be unnecessary to draw any special distinction between the oils manufactured for that

purpose from different materials; and, in considering the characters which are requisite in these products, they need be contrasted only in regard to certain minor details, that are more consequences of the methods of manufacture practised, than of any more essential difference.

By examining the tabular statement of the composition and characters of the various substances constituting both petroleum and the so-called coal or shale oils, and considering what are the conditions under which the oils prepared from these materials are to be used for lighting purposes, it will be evident that there is only a portion either of the natural or artificial material that is applicable as oil for burning in lamps.

Leaving out of consideration the first four members of the series, which are gaseous at ordinary temperatures, it will be seen that the remaining members of the series, which are liquid at ordinary temperatures, gradually increase in specific gravity, varying in this respect to the extent of 6¼ pounds per gallon to about 8¾ pounds per gallon. Beyond this latter point the other members of the series are solids, and come under the general denomination of paraffin.

Another important point of difference consists in the boiling point of the individual members of the series. This varies from 87° Fahr. to upwards of 600° Fahr. It is to this character that attention requires to be chiefly directed in regard to the degree of safety with which these substances can be used for burning in lamps. I say the *degree* of safety, because I am far from being disposed to agree with the opinion that has been put forward that the use of any of these substances is necessarily attended with danger. On the contrary, I am strongly inclined to think that all or any of them may be used without any necessary risk being incurred. Of course, due precautions must be observed in the use of these substances, and those precautions must be greater than are requisite in the case of the oils that were formerly used as lighting materials, viz: the fat oils obtained from animals, fish, and plants. Hydro-carbon oils are, by their nature, much more combustible than fat oils, at the same time that they possess higher illuminating power, and it would be by no means prudent to deal with the one in the same manner as with the other.

The outcry that has been raised against the dangerous character of hydro-carbon oils is, however, in no degree more deserving of respect than that which was raised on the same visionary ground when the use of gas was first introduced. All that is really necessary to avoid accident with the hydro-carbon oils is, that those who use them should be properly instructed as to the way in which they should be stored and used. By this means it will be far more likely that the desired result of preventing accident may be secured, than by exciting a vague and unreasonable apprehension of danger.

In regard to the safety of hydro-carbon oil used for burning in lamps, the main consideration is its capability of giving off vapor spontaneously at the ordinary temperature, or when the oil becomes slightly warmed. In regard to this character the various liquid members of the substances given in the table differ considerably. Taking

the first of those that are liquid at the ordinary temperature, it will be seen that its boiling point, which is the thermometric indication of its degree of volatility, is 86° Fahr., a temperature so little above that which may exist in a room where lamps are used, that it would be very objectionable to use such a liquid as a lighting material in that way. The temperatures given in the table as the boiling points of the individual oils are not by any means the lowest temperatures at which they evolve vapor, and for this reason, if we fix 80° or 90° Fahr. as the highest temperature to which the oil in a lamp is capable of being raised, even in an extreme case, the boiling point of a liquid that would be suitable for burning in lamps, under such conditions, ought not to be less than 200° Fahr. above that temperature.

With the lamps now generally in use for burning hydro-carbon oils, it would be only under very exceptional and improbable circumstances that the oil in the lamp could become heated to a temperature so high as 80° Fahr., and even then some of the more volatile portions of these oils, boiling at 160° Fahr., might be burnt without any danger.

But the hydro-carbon lamp oils are never distinct substances in a chemical sense, like the several substances whose characters are described in the above table. They are always mere mixtures of a number of those substances, and consequently they do not present any constant point of ebullition, while the specific gravity and other characters are the mean of those appertaining to the several substances present in the mixture, and that mean also varies according to the relative proportions of the mixed substances.

The absolute separation of these substances is, indeed, a matter almost of impossibility, and, so far as relates to their practical application, not by any means requisite. It is, however, essential that in preparing hydro-carbon oil for use in lamps, the manufacture should be conducted in such a manner as to secure the separation of the more volatile portions, to such an extent, that the oil, when finished, may be heated to 100° Fahr. without taking fire when the flame of a match is brought into contact with its surface in an open vessel such as a saucer. Oil that will bear this test is, I believe, perfectly safe for all reasonable use; and I am of opinion that this simple test is by far the best criterion of the proper character of such oils. While, of the oils constituting petroleum and coal oil, those which have a boiling point below 280° Fahr. would be too volatile for ordinary use in lamps; on the other hand, those which have a boiling point above 600° Fahr. are also unfit for this purpose, but for very different reasons. It has commonly been supposed that the reason why these latter oils do not burn well in the ordinary lamps is, that they are too thick, and have not sufficient capillarity to rise in the wick and feed the flame. I am disposed to think that this view is erroneous, and am more inclined to the opinion that their defects, as lamp-oils, are to be ascribed rather to the high temperature requisite for their volatilization, and also to the greater density of their vapor. The chief difficulty that is experienced in the use of these oils of high boiling point,

which are generally known as heavy oils, is the charring of the lamp wicks, and the small flame they give.

By reference to the table it will be seen that the oil which has a boiling point of 527° Fahr., affords a vapor that is twice as dense as that of the oil boiling at 243° Fahr., or, in other words, a given weight of the former oil occupies in the state of vapor only half the bulk of an equal weight of the latter also in a state of vapor. This circumstance alone will, to a great extent, account for the smaller flame of the heavier oils as compared with that of the more volatile oils, whose vapors are less dense. Then, in proportion as the volatility of the oils decreases, or as the temperature requisite for their volatilization increases, at the same time a greater degree of heat is necessary to effect that decomposition of the vapor into gas, which is an essential step in their combustion for lighting purposes. The temperature necessary to effect these changes is so high, that at the same time the wick itself gradually becomes charred and incapable of exercising that capillary action by which the flame can be fed with oil. In addition to these circumstances, it must also be remembered that in proportion as the density of the oil vapor increases, so does the amount of carbon in the vapor increase, and, at the same time, there is a proportionate increase, both in the heating power and the illuminating power of equal volumes of the vapor. Consequently, in using heavy oil of high boiling point in a lamp, the result is that a very small, intensely hot and highly luminous flame is produced at first, and after a very short time the wick becomes charred and incrusted with the surplus carbon, separated in the flame, to such an extent that it soon begins to decrease in size and to lose brilliancy.

It appears, therefore, that the oils at either extremity of the series are equally unfit for use in the ordinary hydro-carbon oil lamps. But in practice it never happens that any one of the several substances described in the table is used for lighting purposes. It is always a mixture of a number of them that is used. Still, the facts that I have just pointed out in regard to the extreme cases, exercise their influence in the same manner with regard to this mixture. On the one hand, it must not contain any such amount of the more volatile members of the series, as will confer upon the mixture the property of giving off inflammable vapor when heated to 100° Fahr., or enable it to become permanently inflammed when brought in contact with a lighted match while at that temperature. On the other hand this mixture must not contain such a proportion of the heavier and less volatile oils, as would render the vapor forming the flame too dense and too highly carbonaceous, and necessitate too high a temperature for that decomposition of the vapor which is essential to the production of light.

The due proportion of these various conditions is secured in practice by collecting the oils during distillation in fractional portions: that which distils over first containing the oils that are too volatile for use in lamps; the second portion that distils over being the burning oil, and the third portion containing the heavy oils and paraffin.

The precise points at which this separation of the oil into these

three portions, takes place, will depend very much upon the nature of the material operated upon. In this respect there are great differences in the various materials now used as sources of these lighting oils. American petroleum generally requires the separation of some 20 or 25 per cent. of its bulk, which is too volatile for use in lamps. Canadian petroleum does not require more than 10 per cent. to be separated on this account. Coal and shale oils also vary much in this respect, according to the material they are produced from, and the temperature at which the crude oil is distilled from the coal or shale. As a general rule, the crude oil of coal or shale contains a greater amount of the volatile oils, the lower the temperature those materials are distilled at. This, however, is not a disadvantage, since the amount of crude oil obtainable is greater, the lower the temperature of the distillation. Generally speaking, coal or shale oil does not require that more than 5 to 15 per cent. of the bulk should be separated as too volatile to be mixed with the burning oil.

At the other extreme there are also differences between petroleum and coal or shale oils. The burning oils manufactured from the latter sources, as generally met with, have a specific gravity of about 0·825, while the specific gravity of those made from petroleum is rarely above 0·800, and sometimes as low as 0·780.

I do not believe there is any really sufficient reason why there should be this difference in the case of petroleum, which, as I shall afterwards point out, involves a serious disadvantage as regards its illuminating power as compared with that of coal oil. The manufacture of these commodities, however, is still carried on so much more by mere rule of thumb, that under the guidance of sound principles based upon knowledge of the materials dealt with, that defects of this kind cannot be wondered at, and it will probably be some long time before these processes come to be carried on in such a way as to secure the best results both to the producer and the consumer.

(To be Continued.)

The Magnesian Light.

From the London Chemical News, No. 227.

Photographers are indebted to the perseverance of M. Sonstadt for the removal of a great obstacle. Every one knows the difficulty which has hitherto been experienced in getting a powerful actinic artificial light. Such a light is, however, furnished by the combustion of the metal magnesium, and, thanks to M. Sonstadt, this metal is now procurable at a price which makes it available for practical purposes.

Magnesium is an easily inflammable metal. A wire of considerable thickness can be ignited in the flame of a candle, and the light evolved by the combustion is of great intensity. It has been ascertained that a wire of 0·297 millimetre diameter will give as much light as twenty-four stearine candles of five to the pound. The powerfully actinic character of the light has been recently demonstrated by Mr. Brothers, of Manchester and Mr. Sydney Smith, both of whom have produced good pictures by its use.

The metal is neither ductile nor very malleable. It cannot be drawn but by employing a method devised by Dr. Matthiesen; it can be forced in a softened state through a small opening in an iron cylinder, and thus strands of wire of considerable length can be formed. The wire has been found to burn more steadily when three or four strands are twisted into a rope; and a simple clock-work arrangement will deliver such a rope to a spirit or oil lamp, in the flame of which it may be burned.

We look for important results from the use of this light. The opportunities for its use are numerous; and we may expect our collections to be soon enriched with pictures of objects hitherto shut out from photographers.

Some are talking widely of "night pictures," as though they expected, by means of magnesium, to obtain a picture of the gloomy effects of midnight on a scene. The principal use of the light will, of course, be for dark interiors; and we hope soon to see the magnificent grottoes of Adelsberg and Antiparos—which the pencil is as powerless to draw as the unaided camera to depict—revealed as brightly as the caverns in the glaciers, so well known to photographers. Another Frith may also show us the wonderful passages in the interior of the Pyramids more clearly than they have ever been seen by the traveler with the help of the two or three candles which light his way through the dark labyrinth, and enable us at our own fire-sides to gaze with awe on the vast range of subterranean tombs at Serapeum. All these and many more objects are now open to an enterprising man, who will no doubt soon be found to avail himself of them.

On the Double-cylinder Expansive Steam Engine. By Mr. Wm. Pole.

From Newton's London Journal of Arts, August, 1863.

The greatest advances made in the improvement of the steam engine, as an economical means of obtaining motive power, have resulted from the application of the principle of expansion, the advantages of which are now well known and universally appreciated among engineers. This principle has hitherto been applied to the greatest advantage in engines with a single cylinder, used for pumping purposes, as in Cornwall. In these cases, the peculiar nature of the motion admits of the steam being cut off after a small fraction of the stroke has been passed over, and allowed to expand during the remainder. When, however, the principle of expansion is applied in this mode to engines for producing rotary motion, some difficulties arise, which limit considerably the extent that the expansion may be carried to, and therefore reduce in a corresponding degree the economy attained.

The double-cylinder engine offers a mode of applying the expansive principle to a rotary motion, which removes, or, at least greatly mitigates, the objections to the single-cylinder: and it is the object of the present paper to state the nature of the advantages of the double-cylinder engine, to explain the principles on which they are based, and to show how these principles have been carried out in pactice with satisfactory results.

The original invention of the double-cylinder engine is intimately connected with the discovery and first application of the principle of expansion itself; but the unfortunate disputes which for a long time prevailed in reference to this subject have somewhat obscured the history. Having had occasion, however, some years ago, to investigate the matter, the author believes the following account represents the facts as accurately as they can be ascertained.

The double-cylinder engine was invented by Jonathan Hornblower, a mechanical engineer of considerable eminence in Cornwall, who took an important part in the application of the steam engine in that district, during the early part of Watt's career. The idea appears to have occurred to him early in the year 1776, if not before. He experimented upon a large working model, the cylinders of which were 11 and 14 inches in diameter respectively; and he published his invention in 1781, describing it as consisting in the employment of two steam cylinders,—the steam after it had acted in the first cylinder, being employed a second time in the other, by permitting it to expand itself; the two cylinders being connected together by suitable steam ports and valves. At the same time, he described also, shortly but clearly, several other inventions relating to the steam engine. One referring to surface condensing, which is so much applied in modern days; others to means of getting rid of the air and condensing water; and another invention was a steam piston, which, altered into a steam stuffing-box, is in common use at the present time. Here, therefore, was clearly developed the theoretical principle, as well as the practical application of the expansion of steam; and it is beyond dispute, that the first publication of the principle to the world was this of Hornblower's. The discovery of this principle, however, is usually ascribed to Watt, on the strength of a letter written by him to his friend Dr. Small, of Birmingham, as early as 1769,—twelve years before Hornblower published his description; in that letter Watt gives a clear and explicit description of the general principle of expansion. Notwithstanding, however, the large practice Watt had about this time, it does not appear that he ever applied expansion with any view to economy in its use till 1776, when an engine at the Soho works was altered to work expansively. In 1778, another engine, at Shadwell was experimented upon; and Watt published his invention with reference to expansion in 1782, eight months after the publication of Hornblower's. The over-zealous friends of Watt, who, in a spirit so contrary to that of the great man himself, have sought to exalt his fame at the expense of another's, have charged Hornblower with pirating the principle, from surreptitious information of Watt's experiments; but no proof was ever given of the accusation. It is not only highly improbable in itself, but is altogether negatived by the fact that the originality of the invention on Hornblower's part was expressly admitted by Watt himself. It may therefore be concluded that the discovery of the expansive use of steam—one of the most important and valuable principles in the whole range of practical science—was original both with Watt and Hornblower; and, although Watt has established the priority of the idea, the first publication of it to the world was made by Hornblower in the double-cylinder engine.

After some years of delay, Hornblower proceeded to manufacture his engines in Cornwall; and the miners, perceiving that the double-cylinder engine acted tolerably well, took advantage of it somewhat largely; and in some cases endeavored to make it supersede Watt's single-cylinder expansive engine, which had also then been brought extensively into use. But as it was impossible to make Hornblower's engine work well, without using Watt's separate condenser, invented in 1769, the competition could not be kept up, and the double-cylinder engine consequently for a time fell into disuse.

Both Watt and Hornblower had failed to perceive that, to work the principle of expansion to its full advantage, it was necessary that the steam should be admitted to the cylinder, in the first instance, at considerable pressure. Down to the year 1814, the pressure of the steam in the Cornish engines never much exceeded that of the atmosphere; and so little economy resulted in practice from the application of expansion with this initial pressure, that it was found scarcely worth using at all; indeed, after Watt's immediate connexion with the district ceased, expansion was rapidly becoming disused and forgotton. The merit of rescuing it from this neglect belongs to two Cornish men—Richard Trevithick and Arthur Woolf—who, both about the same time, introduced into their native district the true means of advantageous expansion, namely, the use of high pressure steam. Trevithick applied this to Watt's single-cylinder engine; Woolf applied it to Hornblower's double-cylinder engine. The two forms of engines thus for the second time became rivals, and competed well with each other for many years; but it is only with Woolf's modification that the author is now concerned.

Woolf published his invention in 1804, while residing in London. It consisted simply in the application of high-pressure steam to Hornblower's double-cylinder engine, which he also made double-acting, to fit it for rotary motion, after the example set by Watt long before. It is obvious, therefore, that the name "Woolf's engine," by which it is so often designated, is quite erroneous. The engine is entirely and solely Hornblower's invention, and there is no more ground for calling it Woolf's than for calling the present Cornish engine Trevithick's: for Trevithick made the same change with this latter engine that Woolf did with the former, yet no one would, on that account, think of disconnecting Watt's name from his own engine; and on the same ground Hornblower ought not to be deprived of the credit which the association of his name with his own invention should secure to him. Woolf's ideas respecting the laws of the expansion of high-pressure steam were very crude, and it is difficult to conceive how a man of such excellent practical knowledge could have deluded himself into the belief of theories so palpably absurd as those he laid down, upon which he based his statements as to the proposed advantage to be derived from the use of high-pressure steam. But, although he was so essentially in error on points of theory, he was not wrong in foretelling that much advantage might ultimately be gained by the use he proposed to make of high-pressure steam; as was proved beyond a doubt when his engines came to be fairly tried. His strong point was, skill

in mechanical detail; and his improvements of the engine in this respect were almost innumerable, for there was scarcely a single part which did not receive some beneficial alteration at his hands. Woolf's first engine was erected in 1806, at Meux's brewery, in London, to which establishment he was engineer, and subsequently others were fixed in various manufactories; but these did little more than serve him as experiments, until 1813, when he returned to reside in Cornwall. Here he found a wide field open for his improvements; he entered in earnest into the manufacture of the engines, and they were highly successful. The new doctrine of high-pressure steam produced quite a revolution in the consumption of fuel there; for he at once raised the duty from about 20 millions, at which Watt had left it (that is, 20 million lbs. raised one foot high with the consumption of one bushel, or 94 lbs. of coal), to between 50 and 60 millions, thereby saving two-thirds of the fuel employed.

But though Woolf was so successful, the Cornish engineers shortly began to see that Trevithick's plan of using high-pressure steam expansively in the single-cylinder engine promised equally good results with Woolf's, and at the same time got rid of the objectionable complexity of the double-cylinder arrangement. Trials on a large scale, in which even Woolf himself was persuaded to assist, soon demonstrated this to be true. The more expensive construction began to be abandoned in the mines, and the Cornish engine gradually settled down into its simplest form, namely, a single engine on Boulton & Watt's construction, but with Trevithick's high-pressure steam and high-pressure boiler; which form it has retained to the present day. Thus, although the double-cylinder engine was the first in which the principle of expansion was originally introduced to the world, and about thirty years afterwards was also the first in which this principle was made effective and advantageous, yet, in both cases, it was ultimately superseded by the more simple form of engine.

It remains now to give some account of the third era of prominence attained by the double-cylinder engine, in its revival at the present day. In this revival many modern engineers have aided; but the author considers it the safest course to confine himself to the statement of his own experience, leaving it to others to give an account of what they may have done.

In 1848 the Lambeth Waterworks Company, on the advice of their engineer, Mr. James Simpson, took the bold measure of proposing to remove their source of supply to the banks of the Thames at Long Ditton, above the tide way; and, as a part of this scheme, it became necessary to force the water, by steam-pumping power, along a cast iron main, 9 miles long and 30 inches diameter, from the source to the reservoirs at Brixton Hill. This problem was a difficult one, no experience on so great a length of large main having then been obtained. The great mass of water in motion along the main, combined with the fragile nature of the cast iron, rendered it essential that the motion should go on in the most equable manner, and that concussions or irregularities of pressure should be as much as possible avoid-

ed, otherwise frequent fracture of pipes, fraught with serious consequences to the district they passed through, might be looked upon as almost certain. At the same time, from the large steam power required, it became necessary that all possible improvements in regard to economy of fuel should be adopted. At that time the Cornish single-cylinder expansive engine, which had been introduced into London by Mr. Wicksteed, had been somewhat extensively tried for waterworks purposes, and had justified its well known Cornish reputation for economy; but as grave objections appeared to present themselves to its use in this case, on account of the irregularity of the single action, it was determined to ascertain whether the other form of expansive engine, the double-cylinder, would not prove more applicable; and since the importance of the case required the most careful consideration, the author was commissioned, in conjunction with Mr. David Thompson, to investigate the subject generally, with a view to the advantageous attainment of the desired economy.

In commencing this investigation it was found that the double-cylinder engine had already been to some extent revived, and that modern examples of it, some of considerable size, were working in various parts of the country. These were visited, and their action carefully examined; but it did not satisfactorily appear that any engines then met with were sufficiently favorable instances of the application of the expansive principle. The expansion had not been carried to a sufficient extent to produce great economy, nor arranged in the best manner to attain equality of motion; and the arrangement of the valves and passages were generally so defective as to cause great loss of power and waste of fuel. Notwithstanding these unfavorable results, however, an attentive study of the principles of the engine led to the conclusion that, with a well-considered design carefully carried out into practice, the double-cylinder arrangement promised not only to be eminently suited to the case in question, but also generally to offer a more beneficial application of the principle of expansion to engines of rotary motion than could be attained with a single-cylinder. In accordance with these views, when the Lambeth Waterworks Extension scheme was carried into effect, four large double-cylinder engines were designed of 600 total horse power, the working of which has fully justified the expectations entertained of their advantages; their use has been speedily and largely extended to other cases; and the soundness of the principle on which they were constructed may now be said to have been fully proved.

The general theory of the double-cylinder engine is so well known, that it is unnecessary to repeat it here: the author proposes, therefore, to confine his remarks to such points as are of interest and importance in elucidating the advantages of this form of engine.

In the first place, in comparing the double with the single-cylinder engine, it is a mistake to suppose that there is any theoretical advantage on either the one side or the other, in regard to the economical effect of the expansion. It was shown by Watt, in an ingenious way, at a very early period, and it is demonstrated in an appendix to this

paper, that theoretically, if the steam be expanded to the same extent, the economical advantage to be derived from the expansion will be precisely the same, which ever form of engine be adopted for the application. And it further results from the principles of the engine, that, for a given initial pressure of steam and a given degree of expansion, the power of the engine, measured by the work it will do in each stroke, depends on the size of the large cylinder only, and is precisely the same as that produced in a single expansive cylinder of the same content. The small cylinder has no effect in adding power, but is merely an appendage, useful only for modifying the arrangement of the expansion and equalizing the steam's action during the stroke.

The important objection, however, against carrying expansion to any great extent in a single-cylinder for rotary motion, is the great irregularity of pressure at different parts of the stroke. For example, if the steam be expanded in a single-cylinder to six times its original volume, by cutting it off when one-sixth of the stroke has been passed over, the motive force acting on the piston will be six times as great at the commencement of the stroke as at the end.

It is evident that the effect of the great excess of pressure will be to give a heavy blow to the piston at the beginning of every stroke, which must produce violent concussions through the whole of the machinery; and tend to produce much mischief and inconvenience in the working. In proportion as a greater degree of expansion is used, the evil will be greater. For example, if ten times expansion be used, the force of the blow at the commencement will be 303, while the mean pressure in the cylinder is still only 100, as before. For this reason in single-cylinder engines it has been found difficult to carry the degree of expansion, and the consequent economy, to the same extent with the rotary as with the single acting pumping engines. In the latter, the piston and all its connexions are free to move under the action of the steam pressure; and therefore the excess of pressure at the commencement of the stroke is at once absorbed in giving velocity to the mass, and does no further harm. But in a rotary engine, the piston being controlled in its motion by the crank and fly-wheel, resist the violent impact, which occurs at the point when it is least able to give way; and the consequence must inevitably be a violent strain, repeated many times every minute, which must ultimately have a prejudicial effect upon the machinery. The advantage of the double-cylinder engine is, that it mitigates this evil; for when its principles are properly understood and applied, it enables the economical benefit of a high degree of expansion to be obtained with much less irregularity of pressure than in the single-cylinder.

In the original double-cylinder engines of Hornblower and Woolf, the steam was allowed to act first in the small cylinder, at full pressure, throughout the whole of its length, and then to expand into the larger one; the proportionate cubic content or the two cylinders thus defining the degree of expansion made use of: and, it is believed, that this method of working was that most commonly used down to the time of the investigations already referred to in 1848. But an inquiry

into the principles of action of the engine, shows that it is most advisable not to allow the steam to enter the small cylinder during the whole stroke, but to cut it off after a certain portion of the stroke has been passed over, and to allow the expansion to commence at that point. And it is an important fact, which the author believes not to have been known until published by himself and Mr. Thompson, in 1851, that there is a certain point of the stroke, depending on the degree of expansion made use of, at which it is more advantageous to cut off the steam than at any other; for the reason that the irregularity in the motive power, which it is so desirable to mitigate, is then reduced to a minimum.

For example, if the mean motive power of the engine be represented by 100, and the extent of expansion adopted be six times, then, in a single-cylinder engine, the initial blow at the piston at the commencement of the stroke, as previously stated, will be represented by 215. In a double-cylinder engine, if the steam is allowed to enter during the whole of the stroke of the small cylinder, the initial blow will be the same in amount as in the single-cylinder engine, namely, 215; the duration of the blow, however, is only momentry, as compared with the first example, where it continues through one-sixth of the stroke. But if the steam is cut off in the small cylinder at 41 per cent. of the stroke, the initial blow is reduced to only 140: and this is the minimum blow that can be obtained with the expansion of six times; for on cutting off earlier, at 25 per cent. of the stroke, the initial blow is increased again to 161.

It thus appears that, as regards a degree of expansion of six times, if this expansion be effected in a single-cylinder, the machinery of the engine will have to bear a sudden blow at the commencement of the stroke, as much as 115 per cent. greater than the mean force due to the effective power of the engine. Next, if a double-cylinder engine be employed, and the steam be allowed to enter during the whole stroke of the small cylinder, but little improvement is effected: the blow at the commencement is as great as in the single-cylinder, only lasting a shorter time. But if the same engine be so arranged that the steam is cut off in the small cylinder at the proper point of the stroke, the initial blow may be reduced from 115 per cent. to only 40 per cent. in excess of the mean force; and thus a real and most beneficial improvement may be effected in the action of the engine. This most advantageous point of the stroke for cutting off, the author determined by calculation, in an appendex to his paper, which showed that it varies with the extent of expansion adopted in the engine.

The following Table shows the best point of cut off under various degrees of expansion, with corresponding results in the double-cylinder and single-cylinder engines. The first column gives the number of times the steam is to be expanded; the second shows the percentage of the stroke at which the steam will best be cut off in the small cylinder; the third, the corresponding proportionate area of the small cylinder in percentage of the large one, the length of stroke being the same in both; and the two last columns show the comparative advan-

tage of the double over the single-cylinder engine, in respect to the excess of the initial blow over the mean motive force. The calculation of these results is explained in detail in the appendix.

Table, showing best point of cut off in double-cylinder engine with different degrees of expansion, and comparative initial blow in double and single cylinder engines.

Number of times steam is expanded.	Best Point of Cut off in small cylinder. Percentage of stroke.	Capacity of small cylinder in percentage of large cylinder.	Comparative Initial Blow; the mean motive force being 100	
			Double cylinder engine.	Single cylinder engine.
	Per cent.	Per cent.		
4 times	50	50	126	168
6 times	41	41	140	215
8 times	35	35	151	260
10 times	32	32	161	303

A comparison of the two last columns shows that, for a high degree of expansion, such as eight or ten times, the excess of the initial blow over the mean force of 100 is less than one-third as great in the double-cylinder as in the single-cylinder engine. It is clear, that the amount of this initial blow is the maximum strain on the whole of the machinery, by which the steam power is transmitted from the piston to the fly-wheel, and it, consequently, determines the strength of the various parts necessary to resist this strain. Hence, in proportion as the initial blow can be reduced, all these moving parts are required to be less massive in construction; and all are subject to much less violent causes of fracture and derangement in their working.

Another point of improvement, to which the attention of the author and Mr. Thompson was prominently directed, was the arrangement of the valves and steam passages in the double-cylinder engine. In the engines they had the opportunity of examining, the system of valves commonly used was not only complicated, inconvenient, and expensive in construction, but also wasteful in action; the great size and disadvantageous arrangements of the passages caused considerable waste of steam, and consequent loss of power and fuel. There is a pecularity in the double-cylinder engine, in its requiring a pipe or passage of some kind, through which the steam must travel, from one end of the small cylinder to the opposite end of the large one; and this passage should evidently be as small in content as possible, consistently with allowing the free passage of the steam. When the communication is opened at the end of the stroke, the steam, passing from the small cylinder, has to expand and fill this passage before it enters the large cylinder; and if the passage be large, the steam must necessarily suffer a reduction of pressure in so doing, which must seriously diminish its effective action on the large piston during the future stroke, and so cause much loss. In some engines, the author found this loss so great as to waste

nearly half the power of the engine; and even in the best that were examined, such a considerable percentage of loss occurred, as almost to neutralize the benefit of expansion altogether.

Much attention was therefore given to this point in designing the new engines; and it was found essential to the success of the engines that some arrangement of valves should be adopted which should satisfy the following conditions: first, that they should be of the simplest possible character, and free from liability to derangement; second, that they should admit of the steam being cut off from the small cylinder at such a point as might be necessary to secure the required regularity in the motive power of the engine; and, third, that they should give the clearance spaces the smallest content possible, and, in particular, should allow the passage between the two cylinders to be direct and unimpeded, and of no larger capacity than was absolutely necessary for the passage of the steam. It was further found conducive to economy that the passage between the two cylinders should, if possible, never be opened either to the high-pressure steam or to the condenser, and should, moreover, be carefully protected from cooling.

A construction of valve was accordingly introduced which combined all these conditions, and was found in its practical working to be very satisfactory.

Proc. Inst. Mech. Engineers.

Gun Cotton—Important Application.

From the London Mining Journal, No. 1500.

At the Royal Institution, on Friday evening, the 13th inst., the recent improvements in Gun Cotton again formed the subject brought to notice, the lecturer on the occasion being Mr. Scott Russell. In the previous lecture on the subject, by Mr. Abel, the chemical history of gun cotton, and the nature of the improvements in its manufacture recently made in Austria, were more especially dwelt on; while Mr. Russell principally directed attention to its use. He stated that at a late meeting of the British Association a committee was appointed, of which he was a member, to investigate the nature of the great improvements which it was known had been made in Austria on gun cotton by General Leuz, but which were there kept a profound secret by the Austrian Government. One of the first things the committee did was to write to General Leuz, to request him to come over and acquaint the committee in what his improvements consisted. This impudent request—as Mr. Russell admitted it to have been—excited a commotion in Vienna, but the young Emperor, against the advice of his counsellors, consented to allow the secret to be known, and Gen. Leuz came to this country, and informed the committee what it was. The object to be accomplished in the manufacture of gun cotton, to render it applicable, was to diminish the rapidity of its combustion, and its liability to accidental explosion. These objects have been fully accomplished, as was previously stated by Mr. Abel, principally by spinning the cotton into yarn before it undergoes the process of conversion into gun cotton; and it was shown by that gentleman that by mechanical alteration of the structure of

the cotton fibre alone the explosive material may be placed under complete control, and that, as he said, you may do what you like with it. What can be done with gun cotton, and what cannot be done with it, and its merits, compared with gunpowder, as an explosive agent, were explained and illustrated by Mr. Scott Russell, who, though he commenced by mentioning some points of objection to gun cotton, finally removed them, and represented it to be *in every respect superior to gunpowder*, and to be capable of producing far more destructive effects.

As an illustration of the different degrees of rapidity of combustion attainable with gun cotton, he first ignited a thick yarn of the substance that was suspended round the gallery of the lecture-room, which burned along at the rate of about one foot in a second. He next took a pipe about a yard long, which flashed off in half a second; and another of similar length, of the most explosive kind, exploded instantaneously. A remarkable property of gun cotton, which constitutes an immense advantage in its use compared with gunpowder, is that it leaves no products of combustion behind and produces very little smoke. Another important advantage is, that it is not injured by moisture, and it may be wetted and dried again without impairing its power. This was shown by immersing a piece of gun cotton yarn in water, which was then squeezed out, and, though still damp, the outer surface rapidly ignited, and when the remainder was squeezed the cotton again ignited. This property is a great safeguard in the manufacture of gun cotton, for all the processes may be conducted under water, and it is afterwards dried. For ordinary practical purposes the rapidity of its explosion is reduced to an equality with that of gunpowder, in which state 1 lb. propels a projectile from a cannon with the same velocity as 3 lbs. of gunpowder. The recoil of the cannon when gun cotton is used is only two-thirds the amount of recoil with gunpowder; there is no fouling of the gun, which remains also comparatively cool; and the absence of smoke enables the gunners to continue firing in casemated batteries and between the decks of ships without annoyance. As an explosive agent, gun cotton possesses some remarkable pecularities, the causes of which have not yet been ascertained. A piece of loose gun cotton fired on a balanced scale produces depression, the whole force going off into the atmosphere without any recoil. But if the gun cotton be enclosed in a box or barrel, its explosive force in all directions is much greater than that of gunpowder. Some important experiments on a large scale have been tried with gun cotton made by a manufacturer of the substance in Suffolk. A pallisade was formed with the trunks of a number of large trees fixed in the ground close together, and a small box filled with gun cotton was exploded near them. A large gap was instantly made, the trees having been cut asunder quite straight, though at the top of the fractures the wood was splintered into matches. Its destructive effect was tried on two wooden bridges, which were completely splintered; but the most important experiment was the blowing up of a ship of 400 tons burthen by means of 40 lbs. of gun cotton enclosed in a barrel and placed under water at a distance of 20 ft. The ship, it was stated, was completely blown to pieces.

The explosive force of gun cotton when applied to mining operations is equally effective. At the Clay Cross Collieries it has been found that a blast with 1 oz. of gun cotton will remove from 60 to 80 cubic feet of coal, and it has been applied at the slate quarries in North Wales with equal success. It is a great advantage in using gun cotton in mining or tunneling that the work is not impeded by smoke, which is a serious inconvenience when blasting with gunpowder.

Mr. Scott Russell attributed the remarkable diminution of recoil in explosions of gun cotton to the absence of solid matter in the products of combustion. In gunpowder the solid refuse amounts to about 60 per cent. of the weight of the powder, and the effort required to blow away this "rubbish," he considers, produces the additional amount of recoil. In gun cotton, on the contrary, there is scarcely any solid refuse, 25 per cent. of the products of combustion consisting of water; and he conceives that the expansion of this water into red-hot steam by the ignited gases during their explosion is a main cause of the great explosive force of gun cotton. Mr. Russell ventured also to speculate that the lower degree of heat caused by the explosion of gun cotton may be owing to the subsequent condensation of the steam into water. He admitted, however, that the causes of these and of other peculiar properties of gun cotten are involved in doubt, which he hoped the further investigations of the committee of the British Association would remove. He also mentioned that a commission had been appointed by the Government to consider the application of gun cotton to artillery.

Conversion of Salt Meat into Fresh; a further application of Dialysis,

By A. A. WHITELAW.

From the London Chemical News, No. 234.

As an appendix to the notice of my process for the utilization of brine (published in the *Chemical News* for March 26), I now beg to direct attention to a modification of that process, applicable to ships at sea, by which the quality of the meat supplied to the men may be much improved, and their food varied.

The salt meat is placed in a dialytic bag made of untanned skin, or other suitable material, and the bag filled nearly, but not quite, full of brine from the beef barrel. The dialyser is then placed in sea water, and the process allowed to go on for several days, till the meat and brine are sufficiently fresh for use, or till the brine in the dialytic bag is within 1° or 2° of Twadde's hydrometer of the same strength of sea water. In this way, as the brine becomes freed from salt, the beef, which, by the action of salt, has been contracted, gives its salt to the brine in the bag, and so the process goes on, the beef expanding like a sponge, and gradually taking up a great part of the natural juice that it had previously lost in the salting process. In this way no loss of juice is sustained by steeping, and the brine left in the bags, after a nightly dialysis in fresh water, can be used for soup.

Thoroughly salted beef, without bone, takes up nearly one-third its weight in juice, and this absorption takes place gradually as the strength of the brine in the dialyser becomes reduced.